# PHILOSOPHICAL

# TRANSACTIONS

OF THE

# ROYAL SOCIETY

OF

# LONDON.

FOR THE YEAR MDCCCXLVI.

PART II.

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MDCCCXLVI.

# ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1846 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1843, and prior to the termination of the Session in June 1846.

The Council propose also to give one of the Royal Medals in the year 1846 for the most important unpublished paper in Geology or Mineralogy, communicated to the

Royal Society for insertion in their Transactions after the termination of the Session in June 1843, and prior to the termination of the Session in June 1846.

The Council propose to give one of the Royal Medals in the year 1847 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1844, and prior to the termination of the Session in June 1847.

The Council propose also to give one of the Royal Medals in the year 1847 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1844, and prior to the termination of the Session in June 1847.

The Council propose to give one of the Royal Medals in the year 1848 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848.

The Council propose also to give one of the Royal Medals in the year 1848 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848.

The Council propose to give one of the Royal Medals in the year 1849 for the most important unpublished paper in Physics, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1849 for the most important unpublished paper in Geology or Mineralogy, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

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PHILOSOPHICAL TRANSACTIONS.

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## IV. *The Blood-corpuscle considered in its different Phases of Development in the Animal Series. Memoir* I.—*Vertebrata.* *By* T. Wharton Jones, *F.R.S., Lecturer on Anatomy, Physiology and Pathology, at the Charing-Cross Hospital, &c.*

Received February 27,—Read June 19, 1845.

### Part I.

THE mode in which I purpose to treat this part of my subject, is to trace the blood-corpuscle in its different phases of development in the vertebrate series, from the lower to the higher classes.

I commence therefore with the blood-corpuscle of the Skate, taken as an example of the lowest class.

*Examination of the Blood-corpuscles of the Skate.*

1. There are in the blood of the Skate two kinds of corpuscles, which at first view, under the microscope, especially attract the notice of the observer: the one kind is without colour, but appears dark in consequence of refracting the light strongly, is of a roundish form, and about $\frac{1}{1800}$th of an inch in diameter, or even larger; the other kind, which is more numerous, is of a red colour and oval form, and measures about $\frac{1}{1100}$th of an inch long, by $\frac{1}{1700}$th of an inch broad (figs. 1, 2 *a*, 3, and 7.).

2. *Structure of the first-mentioned kind of Corpuscle.*—The first-mentioned kind of corpuscle is composed of an agglomeration of granules surrounded by a cell-membrane. The granules are clear and strongly refract the light. They measure about $\frac{1}{20.000}$th of an inch in diameter, but it is to be remarked, that they may at one time appear separate, and at another time more or less fused together, as if they had been granules of quicksilver.

3. In consequence of the structure, so far as it has been described, of the corpuscle under consideration, I propose to designate it by the name of *granule blood-cell.*

4. On one side of this granule blood-cell, a clear spot may sometimes be seen, indicating the place of a nucleus (fig. 1.).

5. By the action of water, the cell-wall becomes distended so that it is enlarged in diameter; at the same time the aggregation of the granules becomes less close and their appearance of fusion less; sometimes I have seen the granules suddenly collapse and flatten under the action of water, as if they had been small vesicles bursting (fig. 2 *a*.).

6. By the action of acetic acid much diluted, the granulous mass is dissolved, but the cell-wall is left, and in its interior there is now distinctly seen a cellæform nucleus, about $\frac{1}{4300}$th of an inch in diameter (fig. 2 *b*.). I have watched individual cells during the action of the acid on them, and have seen one granule disappearing after another in the most beautiful and striking manner, until the nucleus came into view.

7. I have above spoken of the granule blood-cell as being of a roundish form: as the blood of the Skate subjected to examination was obtained from the blood-vessels of the viscera already removed from the animal, I cannot say what form the granule blood-cell would present in blood drawn from the animal during life and forthwith examined. But in some examples of the blood which I examined, and which was obtained at a time which could not have been long after the death of the animal, the granule-cells at first presented most remarkable changes of shape and other phenomena immediately to be described, which after a time ceased to be presented, the cells settling into a round form.

8. As the changes of shape and other phenomena to which I refer, resembled appearances presented by what we shall find to be the corresponding corpuscle in the blood of the Frog and of other animals, drawn during life, I am disposed to believe that they will also be found presented by the granule-cells of blood actually drawn from the living Skate.

9. My attention was first attracted to the phenomena by observing a granule-cell with the granules apparently escaping from it as if burst (fig. 3.). But the cell soon appearing again with all the granules collected together, I was led to watch, and soon perceived that the appearance of granules escaping, as if from a burst cell, was owing to this:—the transparent and colourless cell-wall bulged out on one side, leaving the granules still agglomerated and holding together, but this only for a short time; for soon single granules were seen to separate and burst out from the rest, and to enter the hitherto empty compartment produced by the bulging out of the cell-wall. The regular manner in which this sometimes took place was remarkable. I have actually seen the granules enter the compartment by one side and circulate along the bulging cell-wall to the other side, until the whole compartment became filled with granules. This having occurred the bulging began to subside, but was succeeded by a bulging of another part of the cell-wall, into which again a flow of granules took place; and so on all round the cell.

10. *Structure of the Red Oval Corpuscle.*—This is readily recognised to be an oval-shaped cell, with a nucleus round or oval, and with red-coloured contents. It thus resembles the well-known red corpuscle of the blood of the Frog, in structure as well

as in general appearance; the only difference being that its nucleus is cellæform, whereas that of the red corpuscle of the Frog is, as we shall find, rather solid-looking. The red oval corpuscle of the blood of the Skate under consideration, I propose to name *nucleated blood-cell*, in contradistinction to the granule-cell, which, though also nucleated, is not in general discovered to be so until after the solution of the granules by acetic acid, and which in consequence of its being filled with granules is otherwise well designated granule blood-cell*.

11. Besides the two kinds of cells which I have now described in the blood of the Skate, there are others which, on account of their paleness, do not at first attract so much attention, but which it is of the greatest importance particularly to notice.

12. One kind (fig. 4 *a*.) has the characters of the granule-cell, only the granular contents are very fine and do not so strongly refract the light, hence its paleness. This pale granule-cell I consider an advanced stage of the dark-looking granule-cell. This latter I will call the *coarsely granular stage*, the former the *finely granular stage* of granule-cell.

13. Another kind of the corpuscles referred to, has essentially the same structure as the red nucleated blood-cell above described, being a cell containing in its interior a cellæform nucleus (fig. 5 *a*.); the only difference is that both cell and nucleus are circular (the former about $\frac{1}{1800}$th of an inch in diameter, the latter about $\frac{1}{3000}$th), and that the cell is either destitute of colour or but slightly tinged red.

14. That this circular nucleated cell is an early stage of the oval red nucleated cell is shown by the circumstance, that besides it there are cells having the same structure but more or less oval-shaped, and still either altogether uncoloured, or slightly tinged red (fig. 6.), which evidently form a transition to the oval red nucleated blood-cell.

15. Two stages of the nucleated blood-cell may thus be recognised, an *uncoloured* and a *coloured stage*.

16. The description now given of the nucleated blood-cell of the Skate in the uncoloured stage, and a comparison of figures 5 *a*. and 2 *b*. will show that it resembles the granule blood-cell after its granules have been dissolved by the action of acetic acid; the only difference being that in the former the cellæform nucleus is larger and more developed.

17. This resemblance, and the circumstance that corpuscles occur in which the cellæform nucleus is large, but still surrounded by a few granules (fig. 4 *b*.; see also the corresponding corpuscle in the Lamprey, represented in fig. 2.), and which may be viewed as a transition from the granule to the nucleated cell, go to prove that the nucleated blood-cells are developed from the granule blood-cells—are, in fact, the granule blood-cells with the cellæform nucleus developed, at the expense of the granules which have disappeared.

* The name "granule-cell" I have borrowed from Professor Vogel of Gottingen, who first employed it to designate a form of cell which is developed in inflammatory exudations.

18. The conclusion to be drawn from what has now been said in regard to the relationship between the granule blood-cell and the nucleated blood-cell is, that they are to be viewed as two different phases of development of the same body, the granule-cell being the *first phase of development*, the nucleated cell the *second phase of development*. Each phase of development again comprehends two principal stages: the phase of granule-cell, *a coarsely and finely granular stage*; the phase of nucleated cell, *an uncoloured and a coloured stage*.

19. In the osseous fishes which I have examined for the purpose, the blood-corpuscle in its phases and stages of development is essentially the same as in the Skate. In the Lamprey also, which is a cartilaginous fish lower in the scale than the Skate, the blood-corpuscle in its phases and stages of development is the same, with the exception that the nucleated cell in its coloured stage is circular, as well as that in the uncoloured stage (figs. 4 *a*, 3.)*.

20. It is worthy of remark, however, that there is a fish, the lowest of the lowest division, in which the blood is perfectly colourless, viz. *Branchiostoma lubricum*, Costa, *Amphioxus lanceolatus*, Yarrell†. The corpuscles of the blood of this fish have not been examined, but there can be little doubt that some of them are the same as the granule blood-cells of the Skate or Lamprey, and others the same as the nucleated blood-cells in their uncoloured stage. If this be so, the blood of *Branchiostoma lubricum* would differ from that of other fishes, merely in the absence of nucleated cells in the coloured stage. It is quite possible, however, although the blood appears perfectly colourless, that nucleated cells in the very commencement of the coloured stage may exist.

21. From Fishes I now proceed to Reptiles, and as an example of a Reptile I take the Frog, in which to examine the blood-corpuscle.

*Examination of the Blood-corpuscles of the Frog, and a comparison of them with those of the Skate.*

22. In the blood of the Frog, as in that of the Skate, the following kinds of corpuscles are to be recognised:—granule-cells and nucleated cells; the former in coarsely and finely granular stages, the latter in uncoloured and coloured stages.

23. What are usually spoken of under the names of "lymph," or "colourless" corpuscles, are the granule-cell in both its stages, and the nucleated cell in its uncoloured stage. The relation of these corpuscles to the lymph will be considered in Part Second of this paper; here I view them as corpuscles existing in the blood, and therefore as blood-corpuscles: blood-corpuscles, moreover, differing from each other

* Professor Rudolph Wagner was the first to point out the circular form of the red blood-corpuscle of the Lamprey, but he does not appear to have noticed the existence of a nucleus.

† Müller, Mikroskopische Untersuchungen über der Bau und die Lebenserscheinungen des *Branchiostoma lubricum*, Costa, *Amphioxus lanceolatus*, Yarrell. Auszug aus dem Monatsbericht der konigl. Preuss. Akademie der Wissenschaften zu Berlin, Dec. 1841.

in their degree of development, and not to be confounded together under one specific name or epithet, whether "lymph" or "colourless."

24. *Granule Blood-cells.*—The corpuscle in the blood of the Frog, clearly identical with the granule blood-cell of the Skate in the coarsely granular stage, is roundish, and contains clear granules strongly refracting the light, like those of the same cell in the blood of the Skate, only not so large (fig. 1 *a.*). These granules are usually collected in greater number to one side. The cell itself I have seen, on carefully watching it in blood drawn from the living animal, exhibiting changes of shape with movements of the granules, such as have been above described in regard to the granule-cell of the Skate (par. 7 *et seq.*).

25. By the addition of water this granule blood-cell becomes somewhat distended, and if then *very much diluted* acetic acid, or rather to express myself more correctly, water very slightly acidulated with acetic acid, be added, the granules are dissolved and a cellæform nucleus about $\frac{1}{3500}$th of an inch in diameter comes into view (fig. 1 *b.*). If stronger acetic acid is added, instead of one such nucleus, the appearance of two or more smaller ones lying close together is apt to be presented.

26. The granule blood-cell in the finely granular stage of the Frog, when by the action of water it has become distended, presents itself of a regular circular form with a diameter usually about $\frac{1}{1900}$th or $\frac{1}{1800}$th of an inch, but sometimes greater, sometimes less (fig. 2 *b.*). It contains an indistinctly granular matter which does not refract the light much, and therefore it appears very pale.

27. Before being acted on by water, the cell under consideration usually presents itself of an irregular shape with processes shooting out from it (fig. 2 *a.*). After the blood is drawn, it rapidly undergoes the change to this shape, which, it is to be observed, is somewhat analogous to the change of shape which the coarsely granulous blood-cell presents.

28. When by the action of water the cell has been distended, a cellæform nucleus is sometimes already visible in the interior through the finely granular contents (fig. 2 *b.*), but by the action of acetic acid one is always brought distinctly into view. The diameter of this cellæform nucleus in the state of distention in which it is seen, may be put down at about $\frac{1}{3500}$th of an inch.

29. Instead of one cellæform nucleus of this size, an appearance of two, three, or even four smaller ones, may be brought out (fig. 2 *c.*), and this even more readily than in the case of the coarsely granulous cell (par. 25.). I am satisfied, however, that this is in both cases merely an appearance produced by the shrivelling together of the walls of the single cellæform nucleus in consequence of the action of the acetic acid.

30. It is proper to observe, that I have come to this conclusion only after having particularly tested the point by repeated, careful, prolonged and varied observations. Nor is the determination of the point of small moment, as on the appearance of a multiple nucleus which I have thus shown to be artificially produced, and on a similar, but I believe equally artificially produced appearance of a multiple nucleus in the

pus-corpuscle, a particular view has been founded as to the first formation of the nucleus, and indeed as to cell development generally. This subject I hope to consider on another occasion.

31. To return. I have observed that when the blood was treated with water first, so as to distend fully the cells in question, and then the acetic acid added in a very dilute state, never more than a single large cellæform nucleus came into view; whereas if the acid was added a little less diluted, and before the distending action of the water had been allowed to have its full effect on the cell, very generally an appearance of two or more small cellæform nuclei came into view. Though these nuclei have sometimes appeared as if detached from each other, they have been usually in contact, and I have been able to trace the contour of the original single cellæform nucleus from the one small body to the other.

32. By the action of water alone I have, indeed, sometimes seen, in those cells in which the nucleus is already visible through the finely granulous contents, an appearance as if the nucleus were subdivided into smaller, but very generally this has not been the case.

33. The distortion of shape, produced by external agencies, which I have now described as giving a false idea of the real structure of the cellæform nucleus of the granule-cell of the Frog, has its analogue in the now well-known distortion which the primitive tubules of nervous substance so readily undergo.

34. *Nucleated Blood-cells—Uncoloured stage.*—In the blood of the Frog, besides the granule-cells just described, there are seen uncoloured but distinct and well-defined corpuscles, round or oval, finely granulated, and refracting the light to a certain degree, and about $\frac{1}{3300}$th of an inch in diameter.

35. These corpuscles are apt to be confounded with the granule-cells, and appear to have been sometimes taken for free nuclei. They are nuclei but not free*, being surrounded by a cell-wall, which, without a good microscope and good light, is readily overlooked on account of its great paleness. This very pale cell, with distinct nucleus, which is the nucleated blood-cell in the uncoloured stage, may be circular (fig. 4.), or oval (fig. 5.), and of a size approaching that of the red corpuscle, or nucleated cell in the coloured stage.

36. Sometimes the pale cell-wall so closely embraces the nucleus as not to be distinguishable at all until distended and brought out by the addition of water, when it appears as represented in fig. 3.

37. This nucleated blood-cell in the uncoloured stage of the Frog is very similar to that of the Skate, the principal or indeed only difference being that its nucleus has become solid-looking and condensed, so that it has little of the cellæform appearance which the nucleus of the corresponding cell in the Skate presents.

38. *Nucleated Blood-cell—Coloured stage.*—This is the well-known red oval corpuscle of the blood of the Frog (fig. 6.). It is the counterpart of the red oval cor-

* There may sometimes be seen free nuclei (par. 101.), *Note.*

puscle, or nucleated blood-cell in the coloured stage of the Skate, as above mentioned. The principal difference between the cells in question in the two animals, is in the character and appearance of the nucleus; whilst in the red cell of the Skate, the nucleus, though more or less oval, still retains its cellæform appearance; in that of the Frog it is still more solid-looking and condensed than it was in the nucleated cell in the uncoloured stage, and at last acquires the appearance as if consolidated by fusion into a small oval-shaped homogeneous mass.

39. The development of the red oval corpuscle or nucleated cell in the coloured stage from the nucleated cell in the uncoloured stage, can be readily understood to take place by the latter secreting into its interior colouring matter, becoming at the same time enlarged, and all this in the manner above explained in regard to the red nucleated cell of the Skate.

40. But as to the development of the nucleated cell in the uncoloured stage from the phase of granule-cell, this appears to take place in the following manner:—The cellæform nucleus of the granule-cell becomes developed into the solid-looking nucleus at the expense of the granules which disappear. As this takes place, the cell-wall contracts around the nucleus, becoming thicker and stronger, and by and by only becomes distended again by the accumulation of contents, as above described in the case of the Skate (par. 17.)*.

41. The blood-corpuscle of the Frog in its phases of development, thus resembles in all essential respects the blood-corpuscle of the Skate.

42. It would be supererogatory to institute a formal examination and comparison of the blood-corpuscle of any other oviparous vertebrate animal, but for the sake of what comparison may be desired, I have given figures of the blood-corpuscle of a bird in its different phases of development.

43. I now come to consider the blood-corpuscle of Man and the Mammifera in its different phases of development.

In the first place, it is to be observed that, as in the early embryonic state of the Mammifera, the red corpuscles of the blood are most of them very different from the red corpuscles of the blood in a more advanced stage of intra-uterine life as well as after birth, I will, in treating this subdivision of my subject, first examine the blood-corpuscles of the early embryo of a mammiferous animal, comparing them at the same time with those of the oviparous Vertebrata; and then I will examine the corpuscles of the fully-formed blood of Man and the Mammifera, comparing them with those of the early embryonic blood of the same class, and also with those of the blood of the oviparous Vertebrata.

* The probability of such a mode of development of the red corpuscle of the blood of the Frog from the "lymph" or "colourless" corpuscle (granule-cell), has been already well-advocated by Professor H. Nasse and also by Dr. Baly in his translation of Müller's Physiology.

*Examination of the Blood-corpuscles of an Embryo Ox, about 1½ inch in length, and a comparison of them with those of Oviparous Vertebrate Animals.*

44. The blood of this embryo presented the following kinds of corpuscles :—

1. Granule-cells in coarsely granular and finely granular stages.
2. Nucleated cells in uncoloured and coloured stages.
3. Small red corpuscles similar to those of the fully-formed blood of the animal.

45. *Granule-cells.*—Some of these contained pretty large and well-defined granules and refracted the light strongly,—coarsely granular stage (fig. 1.); others presented less defined granules, and refracting the light less, appeared very delicate and pale,—finely granular stage. Fig. 3 represents one of these cells with the cellæform nucleus already visible through the finely granular contents; this may be viewed as a transition to the phase of nucleated cell, whilst fig. 2 represents a granule-cell intermediate between the coarsely and finely granular stages.

46. The size of these granule-cells was from about $\frac{1}{1400}$th to about $\frac{1}{1833}$th of an inch in diameter.

47. *Nucleated Cells—Uncoloured stage.*—In these, as in the corresponding cells of the blood of the Frog, the nucleus, quite cellæform however, was very distinct, presenting a contrast with the scarcely visible cell-wall (fig. 4.). In some, as above described in the Frog (par. 36.), the cell-wall closely embraced the cellæform nucleus, so that this at first appeared free, until by the addition of water the extremely pale cell-wall was brought out. Often the distention of the cell-wall took place on one side only, presenting the appearance delineated in figure 5. In some again the cell-wall was not only distended but pretty distinct, and even slightly reddish (fig. 6.).

48. The size of the cellæform nucleus was from $\frac{1}{4800}$th to $\frac{1}{3650}$th of an inch in diameter, and the cell itself in those in which it was distended from about $\frac{1}{3430}$th to $\frac{1}{1400}$th of an inch in diameter.

49. *Nucleated Cells—Coloured stage.*—Most of these were circular (fig. 7 *a*.), but some were oval (fig. 8.). The circular measured about $\frac{1}{1950}$th of an inch in diameter. The oval measured about $\frac{1}{1400}$th in the long diameter and about $\frac{1}{3133}$th of an inch in the short diameter. The cellæform nucleus, when not at first evident, was brought distinctly into view by the addition of very dilute acetic acid, or even of water merely. It was circular, and measured from about $\frac{1}{4170}$th to about $\frac{1}{3650}$th of an inch in diameter (fig. 7 *b*.).

50. As to the *small red corpuscles similar to those of the fully-formed blood* (fig. 9.), they were rather larger than in the grown animal. They closely resembled the cellæform nuclei of the nucleated cells, only they were red, whilst the latter were uncoloured. There were some free corpuscles however uncoloured which resembled the cellæform nuclei of the nucleated cells, and also the small red corpuscles *minus* their colour.

51. Not having yet had an opportunity of examining the blood on its first appearance in the mammiferous embryo, I do not know if small red corpuscles already

exist then, but in embryos a little further advanced than that, the blood of which is at present under consideration, I found the number of small red corpuscles greater, —that of the nucleated red corpuscles less; and in fœtuses still farther advanced, the well-marked nucleated red corpuscles had altogether given place to the small red corpuscles.

52. Putting the small red corpuscles out of view, the blood of the early mammiferous embryo here examined resembles the blood of the oviparous Vertebrata in presenting, besides granule-cells, nucleated cells both in uncoloured and coloured stages.

53. In regard to the red nucleated cells it is to be remarked, that the circular ones were the most numerous, and as they are identical in appearance, size and structure, with the red corpuscles of the blood of the Lamprey, with the exception that their cellæform nuclei are larger, the blood of the mammiferous embryo might be compared to a mixture of the blood of the Lamprey, with a small quantity from a skate or frog to supply the few oval red nucleated cells, and a small quantity from a calf to supply the small red corpuscles.

54. I now proceed to examine the corpuscles of the fully-formed blood of Man and the Mammifera, and to compare them with those of the blood of the early mammiferous embryo and of oviparous vertebrate animals.

*Examination of the Corpuscles of the fully-formed Blood of Man and the Mammifera, and a comparison of them with those of the Blood of the early Mammiferous Embryo and of Oviparous Vertebrate Animals.*

55. In this examination and comparison, I will inquire,—first, if there be any corpuscles corresponding to granule-cells; secondly, if any corresponding to nucleated cells; and lastly, if the "red corpuscles" are found not to correspond to nucleated cells in the coloured stage, I will inquire into what they correspond to.

56. Here I omit, as I did above in the case of the blood-corpuscles of the Frog, the use of the terms "lymph" and "colourless" corpuscles as altogether destitute of definite meaning, purposing in the second part of this paper to consider, likewise in the case of Man and the Mammifera, what relationship exists between the corpuscles of their blood, usually confounded together under these names, and the corpuscles of their lymph.

57. *Granule Blood-cells.*—The corpuscles in the blood of Man and the Mammifera corresponding to the granule-cells of the blood of the animals above considered, are certain of those commonly spoken of under the name of "lymph" or "colourless" corpuscles—not all, as will be shown below, par. 66.

58. Both coarsely and finely granular stages of the granule-cell may be recognised. In human blood they in general become speedily collapsed after the blood is drawn, and some, especially those in the finely granular stage, may be seen on careful inspection shooting out processes (fig. 2 *a*.) like the same cells in the blood of the

Frog (par. 27.). In this state they look as if they had burst, but such is not the case in general.

59. By the addition of water, the collapsed granule-cells become distended and their appearance is thus distinctly brought out. Fig. 1 *a*. represents the human granule-cell in its coarsely granular stage thus distended; the contained granules, it will be observed, are minute, and on attentive observation they are seen to be in active molecular motion. Fig. 2 *b*. represents the finely granular stage of granule-cell of human blood, distended by water.

60. The granule-cell of human blood is sometimes seen to present, though not very strikingly, a clear spot indicating, as I think, the place of the nucleus, similar to that above represented in the case of the granule-cell of the Skate.

61. When distended by the action of water the human granule blood-cell is about $\frac{1}{3400}$th of an inch in diameter.

62. Fig. 3 *a*. represents the granule-cell, coarsely granular stage, and fig. 4 the granule-cell in its finely granular stage of the blood of the Horse. Figs. 5 *a*. and 6 represent the coarsely and finely granular stages of the granule-cell of the blood of the Elephant. It will be observed that the diameter of these cells is somewhat greater than that of the granule-cell of human blood, but the principal difference is the size of the contained granules in the coarsely granular stage. Whilst in the granule-cell, coarsely granular stage, of the Horse the granules may be estimated in round numbers at about $\frac{1}{15,000}$th of an inch in diameter, those in the same cell of the Elephant are about $\frac{1}{30,000}$th, and those in the human granule blood-cell about $\frac{1}{25,000}$th.

63. If, after the granule blood-cell has been distended by the action of water, very dilute acetic acid be added, the contained granules are dissolved, and a cellæform nucleus brought into view. Fig. 1 *b*. represents a human granule-cell so acted on; fig. 3 *b*. a granule blood-cell of the Horse similarly acted on; and fig. 5 *b*. a granule blood-cell of the Elephant in the process of being acted on by acetic acid. This last drawing was made before the granules were all dissolved, but when the cellæform nucleus was already distinctly in view. In the case of the cells here represented, as indeed in the case of many others, I watched the progress of the action of the acetic acid on the granules and the coming into view of the cellæform nucleus.

64. If, before the addition of the acid, the granule-cell has not been distended by the action of water, and if the acid has not been much diluted, instead of one cellæform nucleus of the size represented, and which in the human granule-cell may be about $\frac{1}{3650}$th of an inch in diameter, but larger in the Elephant and smaller in the Horse, an appearance of several smaller ones variously aggregated may be brought out. This, however, in opposition to what is generally believed, and in opposition to what I myself once believed, I can affirm most positively is merely an appearance artificially produced by the corrugating action of the acid on the walls of the single cellæform nucleus, altogether in the manner above shown to be the case with the cellæform nucleus in the corresponding cells of the blood of the Frog.

65. *Nucleated Blood-cells—Uncoloured stage.*—In human blood to which water has been added, besides the distended granule-cells, cells of the same size may be seen, not containing granules, but presenting most distinctly a single cellæform nucleus (fig. 7.). Such cells resemble the granule-cell deprived of its granules by the action of acetic acid. Moreover, it will be observed that they are essentially similar to the nucleated blood-cells in the uncoloured stage of the blood of the mammiferous embryo and of the oviparous vertebrate animals. In their mode of development from granule-cells, as indicated by the occurrence of cells in transition stages (fig. 10.) as well as in their structure, they are in fact clearly identical. I recognise them therefore as the corpuscles in human blood *in the phase of nucleated cell, uncoloured stage.*

66. In consequence of the collapsed state into which they fall immediately on the blood being drawn (fig. 8.), these uncoloured nucleated cells of human blood are scarcely to be distinguished from the collapsed granule-cells (fig. 2 *a*.) until after distention by water. They are usually confounded with them under the same name of "lymph" or "colourless" corpuscle.

67. In the blood of the mammiferous animals which I have yet examined in reference to the point under consideration, viz. the Horse, Elephant, Paco, Sheep, Goat and Rabbit, uncoloured nucleated cells likewise exist, those of the blood of one animal resembling those of the blood of another, and all resembling those of human blood in all essential particulars of structure. What difference there is consists in the size of the cell, but especially in the size of the cellæform nucleus, as may be seen by comparing the figures 9 and 11 of the nucleated blood-cells of the Elephant and Goat for example; the cellæform nucleus of the former being about $\frac{1}{3000}$th, that of the latter only about $\frac{1}{8000}$th of an inch in diameter.

68. *Nucleated Blood-cells—Coloured stage.*—In the blood of Man unacted on by reagents I have not yet observed such corpuscles, but in the blood of other mammifera which I have examined, especially in the blood of the Horse and Elephant, I have observed them. Fig. 13 represents one of several such corpuscles observed in the blood of the Horse which had been drawn from the animal for a day or two, but to which no water or other reagent had been added either before or during the examination. The transparent and colourless circumferential ring, the optical expression of the transparent and colourless cell-wall doubled on itself, formed a striking contrast with the red-coloured interior of the cell, as did also the uncoloured circumference of the cellæform nucleus. This nucleated cell differed from the nucleated cell in the uncoloured stage in not being collapsed. The one delineated was not plumply distended, but others were. Fig. 14 represents a coloured nucleated cell from the blood of the Elephant more fully distended.

69. Fig. 15 *a*. represents a nucleated cell slightly tinged red, which was observed in human blood, but only after the addition of water; fig. 15 *b*. represents a still more slightly tinged nucleated cell, observed likewise after the addition of water, in the blood of the Horse, and figs. 16 and 17 represent similar ones from the blood of the

Sheep and Paco. These may be looked on as transitions from the uncoloured to the coloured stage of nucleated cell, such as have been above referred to in the blood of the early mammiferous embryo and in that of the oviparous Vertebrata.

70. Having thus demonstrated, besides granule-cells both in coarsely and finely granular stages, nucleated cells, both in uncoloured and coloured stages, in the fully-formed blood of the Mammifera, as well as in their early embryonic blood, though in much smaller number and of a much less deep colour as regards the cells in the coloured stage*, I now come to inquire into the nature of the well-known "red corpuscles" of the fully-formed blood of the Mammifera.

71. "*Red Corpuscle*" *of the fully-formed blood of Man and the Mammifera.*—In consequence of similarity of colour, it has been generally taken for granted that this is the exact analogue of the "red corpuscle" or coloured nucleated blood-cell of the oviparous Vertebrata. Physiologists have accordingly supposed that it should contain a nucleus; but though unsuccessful in the attempt to demonstrate one, they have not altogether ceased to believe in the existence of an exact analogy between it and the "red corpuscle" or coloured nucleated blood-cell of the oviparous Vertebrata; they have rather had recourse to conjecture to account for the absence of a nucleus.

72. In reference to this point, the red nucleated cell of the blood of the early mammiferous embryo has been especially appealed to; it being considered by Mr. Gulliver, and I think correctly, as the exact analogue of the "red corpuscle" or coloured nucleated blood-cell of the oviparous Vertebrata. But to take it for granted that the "red corpuscle" of the fully-formed blood of the Mammifera is the exact analogue of the red nucleated cell of their early embryonic blood, is not more warranted than to take it for granted that the "red corpuscle" of the fully-formed blood of the Mammifera is the exact analogue of the red nucleated blood-cell of the oviparous Vertebrata.

73. Under the impression however that the "red corpuscle" of the fully-formed blood of Man and the Mammifera is the exact analogue of the red nucleated cell of the blood of the early mammiferous embryo, Mr. Gulliver endeavours to account for the absence of a nucleus in the former by supposing that it disappears at an early period of intra-uterine life. This apology for the absence of a nucleus in the "red corpuscle" of the fully-formed blood of Man and the Mammifera, would have had weight if the "red corpuscle" had been an object persistent throughout life like a limb or an eye, but as it is an object constantly disappearing and being regenerated, we should expect, if it were really a nucleated cell originally, to meet with it in a stage when it does contain a nucleus.

* It is proper to mention that an appearance simulating that presented by the circumferential doubling of the cell-wall and by the slightly coloured interior of the nucleated cell, coloured stage, of fully-formed mammiferous blood, may be produced by the diffraction of the light around any corpuscle. As the diffracted shadows referred to however are always concentric with the corpuscle, error from this source may with certainty be avoided by the unpractised observer in all those cases at least, in which the cell-form nucleus is situated eccentrically within the cell.

74. Professors H. Nasse*, Schultz† and Henle‡, who appear to have observed the nucleated cell of the fully-formed mammiferous blood both in its uncoloured and coloured stages as above described, have supposed it to be the "red corpuscle" in such a stage. But its larger size (compare figs. 13 and 20, 14 and 23, 11 and 24) seems to forbid such a supposition. Supposing however that this objection could be waived, still it is nothing but unsupported conjecture to say that the nucleated cell and the "red corpuscle" are *exactly* the same body in different stages of development.

75. On the other hand, there is a chain of evidence which appears to me to point forcibly to a different view of the nature of the "red corpuscle" of the fully-formed blood of Man and the Mammifera, whilst by implication it as forcibly forbids the supposition that the nucleated cell and "red corpuscle" are exactly the same body—the latter the same as the former, only *minus* its nucleus, and with an increase of colour.

76. The view of the nature of the "red corpuscle" of the fully-formed blood of Man and the Mammifera to which I refer is this: the "red corpuscle" of the fully-formed blood of Man and the Mammifera is the cellæform nucleus of the nucleated cell set free by the bursting of this cell itself, and become filled and red by the secretion of globuline and colouring matter into its interior; and the chain of evidence which appears to me so forcibly to point to it is the following:—

77. 1st. *Correspondence in Size between the Cellæform Nucleus of the Nucleated Cell and the "Red Corpuscle."*—Whilst, as we have seen, there is a marked difference in size between the nucleated cell and the "red corpuscle" of the same blood, there is on the contrary a striking correspondence in size between the cellæform nucleus of the nucleated cell and the "red corpuscle." A glance at the figures will show this. In human blood, in which the "red corpuscle" is large, the cellæform nucleus of the nucleated cell is large. In the blood of the Horse, in which the "red corpuscle" is less, the cellæform nucleus of the nucleated cell is also less. Again, in Sheep's blood, in which the "red corpuscle" is still less, the cellæform nucleus of the nucleated cell is still less.

78. These facts, and the fact already pointed out by Mr. Gulliver, that the nucleus of the red nucleated corpuscle of the blood of the early mammiferous embryo is about the same size as the "red corpuscles" in the same blood, which are similar to those of the fully-formed blood of the animal, were sufficient to draw my attention to the correspondence in size between the cellæform nucleus of the nucleated cell and the "red corpuscle," but not sufficient to satisfy me that it was more than a coincidence. I therefore became desirous of testing further the correspondence; and for this purpose it occurred to me that the blood of the Elephant (in which the "red corpuscles" are larger than those of any mammiferous animal, as Mr. Gulliver has

* Article "Lymphe," Wagner's Handwörterbuch. The blood referred to by Nasse was that of pregnant women especially.

† Müller's Archiv, 1839, p. 252. The blood examined was that of the Elephant.

‡ Algemeine Anatomie, p. 444, Pl. IV. fig. 1 E. *c*. The blood referred to appears to be human blood.

shown) and the blood of the Napu Musk Deer (in which, according to the same authority, the red corpuscles are smaller than those of any other mammiferous animal) would be well-adapted.

79. Accordingly I made application to the Council of the Zoological Society for leave to obtain a minute quantity of blood from the animals I have mentioned. With this request the Council of the Society readily complied*, so far as regards the Elephant, but could not as regards the Musk Deer, there being at present no specimen in their menagerie. As a substitute for the blood of the Musk Deer I have had recourse to that of the Goat, as distinguished for very small "red corpuscles."

80. The result of my examination of the blood of the Elephant is, that the cellæform nucleus of the nucleated cell is larger than that of any other animal the blood of which I have examined, and that it presents a remarkable and exact correspondence in size with the "red corpuscle" of the same animal. (See figs. 9 and 23.)

81. The result of my examination of the blood of the Goat is, that the cellæform nucleus of the nucleated cell is smaller than that of any other animal the blood of which I have examined. As to its correspondence in size with the red corpuscles, this does not at first view appear so complete as in the preceding instances; the large majority of "red corpuscles" being smaller than the cellæform nucleus of the nucleated cell. There are, however, some "red corpuscles"† which exactly correspond in size with the cellæform nucleus of the nucleated cell (see figs. 11 and 24), and these have all the characters of the early stage. (See below, par. 86.)

82. Notwithstanding this slight discrepancy in the blood of the Goat, which moreover is rather apparent than real, I believe that it will be admitted as a general proposition, that there is a close and constant correspondence in size between the cellæform nucleus of the nucleated cell and the "red corpuscle" of the same blood.

83. 2nd. *Correspondence in Form between the Cellæform Nucleus of the Nucleated Cell and the "Red Corpuscle."*—In the blood of the animals which I had hitherto examined, I had found the cellæform nucleus of the nucleated cell circular, and therefore so far corresponding in shape with the "red corpuscle;" but not considering this conclusive as to the existence of an essential relation in respect of form, it occurred to me that the point might be tested by a reference to the blood of the Camel tribe, in which the red corpuscles are of an oval shape.

84. Accordingly, availing myself of the liberal permission granted me by the Council of the Zoological Society, I procured a small quantity of the blood of the Paco, and subjected the cellæform nucleus of its nucleated cells to a comparison with its "red corpuscles." The result was that in the nucleated cells generally the cellæform

* I most gladly seize the present opportunity to express my sense of the obligation thus conferred on me by the Council of the Zoological Society.

† The disproportion in size between the nucleated cell and the "red corpuscle" of the blood of the Goat is still greater than that between the nucleated cell and the "red corpuscle" of the blood of the other mammiferous animals which I have examined, and therefore still more strongly forbids the supposition that the nucleated cell and the "red corpuscle" are the same body.

nucleus was circular (fig. 12.). In a few nucleated cells indeed the cellæform nucleus was of a somewhat oval shape (fig. 17.), but this did not seem to be more than what I had occasionally observed in the blood of those Mammifera, in which the "red corpuscles" are circular.

85. My reference to the blood of the Paco thus appeared to decide against an essential correspondence in form between the cellæform nucleus of the nucleated cell and the "red corpuscle." A little further study of the blood of the Paco however soon cleared away this apparent difficulty, and showed me a state of things more corroborative of my view regarding the nature of the "red corpuscle," and in much closer correspondence with analogy perhaps than would have been that which I supposed possible. I discovered, in fact, corpuscles in different stages of transition, from the circular form similar to that of the cellæform nucleus within the nucleated cell, to the oval form of the perfect "red corpuscle;" and I also discovered that in these different stages of transition in respect of form there are different degrees of coloration (figs. 26, 27, 28, 29, 30.).

86. I had already observed progressive degrees of coloration in the circular "red corpuscles" of the Mammifera generally. Thus, in the blood of the Horse, for example, I had observed corpuscles (fig. 19.) similar on the one hand to the "red corpuscle" (fig. 20.) but uncoloured, and on the other to the cellæform nucleus of the nucleated cell (fig. 15.), and this in all respects. Such corpuscles I was disposed to regard as cellæform nuclei recently set free from the nucleated cells, but not yet reddened by the secretion of colouring matter into their interior. Again, I had observed corpuscles similar to those just mentioned but presenting a red tinge in the middle, the circumference untinged appearing like a transparent and colourless ring (fig. 21.); but the combination of progressive stages of coloration of the "red corpuscle" with its progressive changes of form in the blood of the Paco is peculiarly interesting.

87. The transition from the circular to the oval shape presented by the "red corpuscle" of the Paco, is analogous to that which we have seen presented by the red nucleated cell of the oviparous Vertebrata. It is also worthy of remark, that like the latter the oval red corpuscle of the Paco is reduced to a circular form by the action of water (fig. 31.). The progressive degrees of coloration are also analogous to what are presented by the nucleated cells of the blood of the oviparous Vertebrata. But it is curious to observe, that whilst among the Mammifera the attainment of the oval form by the "red corpuscle" is exceptional, among the oviparous Vertebrata the attainment of the oval form by the red nucleated cell is the rule.

88. But to return to the bearing of the facts I have now related on the question before us. They show that originally the "red corpuscles*" of the Paco are of a circular form like the "red corpuscles" of other mammifera, but that whilst the latter remain circular, the former gradually acquire an oval shape. From this it follows that the circular form of the cellæform nucleus of the nucleated cell of the blood of the Paco, so far from being an objection to the view of the nature of the "red corpuscle"

* In their original circular form they are as yet uncoloured.

of the Mammifera which I am advocating, is just what should be according to it. In short, I consider the "red corpuscle" of the Paco, in its circular form and as yet uncoloured state, to be, like the similar corpuscles referred to in par. 86, cellæform nuclei but recently set free from the nucleated cell.

89. 3rd. *Correspondence in Structure between the Cellæform Nucleus of the Nucleated Cell and the "Red Corpuscle."*—The cellæform nucleus of the nucleated cell is a simple cell or vesicle without nucleus or nucleolus, and as such exactly resembles the "red corpuscle" *minus* its red coloured contents. Indeed, when by the action of water the "red corpuscle" is deprived of its red coloured contents without being rendered indistinct, the resemblance in respect of structure is very striking, as indicated by the lumen and transparent circumferential ring—the optical expression of the double contour of the wall of the collapsed cell or vesicle.

90. As already said, "red corpuscles" may occur uncoloured or with the lumen only red, the circumferential ring just referred to remaining transparent and colourless. This indicates that the colouring matter is contained in the interior of the cell or vesicle. In its perfect state however the "red corpuscle" appears red at the circumference also, owing, apparently, to its wall being impregnated with colouring matter.

91. The absence of anything like a nucleus in the "red corpuscle" of the fully-formed blood of the Mammifera is in correspondence with the absence of anything of the kind in the cellæform nucleus of the nucleated cell. The appearance of dots, as if of nucleoli, occasionally seen in the latter, I consider to be mere corrugations, similar to those which give rise to the mulberry appearance which the red corpuscle is so prone to assume.

92. Before admitting the correspondence in size, form and structure, between the cellæform nucleus of the nucleated cell and the "red corpuscle" of the blood of the Mammifera which I have now demonstrated, as evidence in favour of their identity, it will be proper to inquire if there be any points of difference between them sufficient to constitute evidence to the contrary effect.

93. *Points of Difference between the Cellæform Nucleus of the Nucleated Cell and the "Red Corpuscle."*—1st. The cellæform nucleus of the nucleated cell refracts the light more than the "red corpuscle." 2nd. The cellæform nucleus is not affected by water or acetic acid like the "red corpuscles"—the former being by the action of these reagents rendered more distinct, the latter rendered indistinct. These circumstances indicate a chemical difference between the cellæform nucleus and the "red corpuscle," and if a satisfactory explanation of them could not be offered they would constitute a powerful argument against my view. But I believe the following explanation will remove the difficulty.

94. *The objection against the identity of the Cellæform Nucleus and "Red Corpuscle" which might be founded on the different modes in which they refract light, and are affected by Chemical Reagents, explained away.*—The differences mentioned are striking enough between the cellæform nucleus and the "red corpuscle" in its perfect state; but if we take the "red corpuscle" in the stage when, as I have already described,

it is as yet uncoloured or presents colour only in its interior, then we find but little difference between it and the cellæform nucleus of the nucleated cell, either in the mode in which it refracts light, or in which it is affected by water or acetic acid. Even when fully red, the "red corpuscles" are not all equally affected by the action of water or acetic acid; some being always seen which resist to a greater or less degree the action of these reagents. Such corpuscles have been considered to be advanced formations, they appear to me on the contrary to be recent.

95. From this it appears that the "red corpuscle" in its advanced stage does not differ, in the respects under consideration, more from the cellæform nucleus of the nucleated cell than it does from what is undoubtedly itself in an early stage. The differences mentioned therefore argue nothing against the identity of the cellæform nucleus and the "red corpuscle."

96. There being thus no evidence to the contrary effect, the correspondence in size, form and structure, between the cellæform nucleus of the nucleated cell and the "red corpuscle" of the blood of the Mammifera, which I have now demonstrated, will I believe be admitted to constitute pretty forcible evidence as to their identity. Whether any additional evidence can be adduced will be seen in the sequel*, par. 106.

97. If the view of the origin and nature of the "red corpuscle" of the fully-formed blood of Man and the Mammifera which I have now given be correct, this "red corpuscle" must be considered as a third phase of development of the blood-corpuscle, and of which there occur uncoloured and coloured stages. This third phase I propose to call the phase of *free cellæform nucleus*†.

98. The different phases and stages of development of the blood-corpuscle in the Vertebrata, such as I have now traced them, may be summed up thus:—In all the vertebrate animals examined, oviparous, and mammiferous, we find blood-corpuscles in what I call the first phase of development, or phase of granule-cell, this presenting

* The view of the nature of the "red corpuscle" of the fully-formed blood of Man and the Mammifera which I have now propounded, must not be confounded with VALENTIN's view that the blood-corpuscles are nuclei, seeing that he makes no distinction between the blood-corpuscles of the oviparous Vertebrata and those of the Mammifera in this respect. Considering them as analogous to each other, he views them equally as nuclei. The nuclei of the "red corpuscles" of the oviparous Vertebrata he considers to be nucleoli. According to my view, on the contrary, the "red corpuscles" of the blood of Man and the Mammifera are not the analogues of the "red corpuscles" of the blood of the oviparous Vertebrata, but strictly the analogues of their nuclei, as also of the nuclei of the nucleated "red corpuscles" of the blood of the early mammiferous embryo.

Messrs. GULLIVER and REMAK have made observations on the blood of the Horse, from which they have inferred that the "red corpuscles" are formed within the "colourless ones," and that they become free by solution of the latter. GULLIVER however represents colourless corpuscles containing, some one, some two, and some even four "red corpuscles," and these nearly as large as the free "red corpuscles" of the animal. REMAK, in like manner, speaks of one or more "red corpuscles" being formed within the colourless ones. Such appearances I have never observed.

† It appears almost unnecessary to remark, that this view of the nature of the "red corpuscle" of the fully-formed blood of Man and the Mammifera, completely sets aside the vexed question of a nucleus.

in a more or less marked manner two principal stages, a coarsely and a finely granular stage. In all the animals examined we likewise find blood-corpuscles in what I call the second phase of development, or phase of nucleated cell; this again presenting two principal stages, an uncoloured and a coloured. In *Branchiostoma lubricum* alone of all the oviparous Vertebrata, we have seen reason to conjecture (par. 20.) that the nucleated blood-cell occurs in the uncoloured stage only, or if a coloured stage exists, that the coloration is only in a very slight degree.

99. In regard to the nucleated blood-cell in the coloured stage, it is to be observed that it occurs in its highest degree of development and in great number only in the oviparous Vertebrata, in which it constitutes the "red corpuscle," and in the early mammiferous embryo. In the fully-formed blood of the Mammifera it occurs in a comparatively low degree of development, and in very small number.

100. It is in the Mammifera alone that we find the blood-corpuscle in what I call the third phase of development, or phase of free cellæform nucleus*. This exists in both uncoloured and coloured stages. In the former stage it is rare, in the latter it is the "red corpuscle" of the fully-formed blood of Man and the Mammifera. In the early mammiferous embryos which I have examined there have always been, in addition to the numerous "red corpuscles" similar to those of the oviparous Vertebrata, *i. e.* red nucleated blood-cells, some "red corpuscles" similar to those of the fully-formed mammiferous blood. Is there a period in the development of the mammiferous embryo when there exist in the blood no such "red corpuscles," viz. free cellæform nuclei in the coloured stage?

The summary now given will be found both tabularly and pictorially illustrated by the Plate.

## Part II.

In this part of my subject, I purpose to examine the corpuscle of the lymph of vertebrate animals, and to compare it with the corpuscle of their blood.

### *The Corpuscle of the Lymph of the Skate and Mackerel.*

101. The lymph of the Skate examined was obtained from the orbit in very small quantity, and could not be considered a good specimen. The corpuscles were besides a few granule-cells, nucleated cells in the uncoloured stage,—the cell-wall of many extremely pale,—and some nucleated cells in the earlier stage of coloration,—all identical with the corresponding corpuscles of the blood of the animal. (See figs. 1 to 6.)

102. The lymph of the Mackerel, taken as an example of an osseous fish, was likewise obtained from the orbit, and might be viewed as a good specimen. It contained granule-cells, nucleated cells in the uncoloured stage,—many with the cell-wall so pale as to be readily overlooked without attentive examination,—and nucleated cells

* Free nuclei, which may be met with in the blood of oviparous Vertebrata, are to be looked upon as mere abortions of a third phase of the blood-corpuscle.

in the earlier part of the stage of coloration,—all identical with the corresponding corpuscles of the blood of the animal.

*The Corpuscle of the Lymph of the Frog.*

103. The lymph subjected to examination was obtained from one of the posterior lymph hearts. The corpuscles were,—1st. Corpuscles at once recognisable as identical with the granule-cells of the blood of the animal, and these in both coarsely and finely granular stages. There were also cells in transition from granule to nucleated cells. 2nd. Corpuscles which at first sight appeared to be free nuclei, but were discovered on closer examination to be inclosed within cells of a circular or oval form, and extremely pale. The identity of these cells with the nucleated cells, uncoloured stage, of the blood of the animal could not be overlooked. 3rd. A considerable number of red nucleated cells, the same as the "red corpuscles" of the blood. Many of these no doubt were derived from small blood-vessels which had been cut in opening the lymph heart, but some of them I am inclined to believe belonged to the contents of the lymph heart, those especially which appeared to be in an early stage of coloration. (See the figures of the blood of the Frog.)

*The Corpuscle of the Lymph of the Common Fowl.*

104. The lymph subjected to examination was obtained from a lymphatic gland. It contained,—1st. Corpuscles about $\frac{1}{3000}$th of an inch in diameter, less or more, identical with the granule-cells of the blood of the same animal; some being in coarsely and some in finely granular stages (figs. 1 to 2.). 2nd. Corpuscles in much greater number than the preceding, of the form of circular discs, with a depressed point in the centre about $\frac{1}{7000}$th of an inch in diameter, and though colourless very distinct. At first glance these corpuscles appeared to be free, but subjected to more careful examination, they were observed to be contained within a cell of a diameter of about $\frac{1}{3000}$th of an inch, the wall of which was extremely pale. Having once perceived it, however, there was no difficulty in recognising the whole as a nucleated cell identical with the nucleated cell of the blood of the animal, in its uncoloured stage (fig. 3.). There were some nucleated cells similar to those just noticed, but slightly tinged red in the interior; and besides such there were some oval nucleated cells, altogether similar to the oval red corpuscles of the blood of the animal, but coloured only, and that slightly in the interior, the circumferential doubling of the cell-wall appearing quite transparent and colourless.

*The Corpuscle of the Lymph of Man and the Mammifera.*

105. The description of the corpuscles of the lymph, here to be given, was drawn from the examination of those presented by the contents of the thoracic duct. Besides the contents of the thoracic duct of the human subject, I have also particularly examined the contents of the thoracic duct of the Rabbit, and found their corpuscles to be in all essential respects identical.

106. The corpuscles were,—1st. Granule-cells in both coarsely and finely granular stages, altogether similar to those of the blood (figs. 1, 2.). 2nd. Besides many cells in transition from granule to nucleated phase, nucleated cells in both uncoloured and coloured stages, quite the same as those of the blood (figs. 7 and 15 *a*.). The nucleated cells were fully distended independent of the addition of water, and were comparatively the most numerous of the corpuscles in the lymph; those in the coloured stage being more so than those in the uncoloured stage. Slight as the coloration of the coloured stage of the nucleated cell was generally, some few cells presented it perhaps in a more marked manner than is usually presented by the same cells as they occur in the fully-formed blood; in this respect approaching to the red nucleated cell of the blood of the early mammiferous embryo. Some even were of an oval shape. 3rd. A considerable number of free cellæform nuclei, both uncoloured and in different degrees of progressive coloration. These free cellæform nuclei were not much affected by the action of acetic acid or water. Lastly, there were seen here and there among the other corpuscles the empty shell of a nucleated cell in the coloured stage, with a free cellæform nucleus beside it as if just extruded by the bursting of the cell-wall. This is a circumstance which may be viewed as additional evidence, and this of a direct kind, in support of the view above given of the nature and origin of the "red corpuscle" of Man and the Mammifera.

107. From the account now given of the corpuscle of the lymph of vertebrate animals, it appears that it is identical with the corpuscle of their blood. In the oviparous Vertebrata, it occurs, like the corpuscle of their blood, in the two phases of granule and nucleated cell; whilst in Man and the Mammifera it occurs, like the corpuscle of their blood, in the three phases of granule-cell, nucleated cell and free cellæform nucleus.

108. The only difference that exists between the corpuscle in the lymph and the corpuscle in the blood, is, as regards the oviparous Vertebrata, the little degree of coloration which the coloured stage of nucleated cell as yet presents, and as regards the Mammifera, the small degree of coloration which the coloured stage of free cellæform nucleus has as yet attained.

109. The comparatively small total amount of corpuscles in the lymph is well-known. As regards the relative number in the different phases of the corpuscle, it is to be remarked that in the oviparous Vertebrata, the nucleated cells are, as in the blood, more numerous than the granule-cells. The nucleated cells are also more numerous than the granule-cells in the lymph of Man and the Mammifera, which is different from what obtains in their blood. Taking into account this relatively great number of nucleated cells, and the existence, though in less numbers, of free cellæform nuclei, we might compare the contents of the thoracic duct of a mammiferous animal, in respect both of the kinds of corpuscles and of their relative but not absolute numbers, with the blood of its early embryo state. The circumstances above-mentioned, that some of the nucleated cells of the contents of the thoracic duct present in the coloured

stage coloration in a pretty marked degree, and that some are of an oval shape, give additional exactness to the comparison*.

* In the descriptions which have been given by authors of the corpuscles of the lymph much confusion exists. An exposure of the principal causes of this may perhaps prove useful and instructive.

1st. Observations made on the corpuscles of the lymph of oviparous vertebrate animals have been adopted as applicable to the corpuscles of the lymph of Man and the Mammifera, whereas it will have been seen from what has been above said, that the corpuscles of the lymph of the oviparous Vertebrata can no more be taken as an exemplification of the corpuscles of the lymph of Man and the Mammifera, than the blood-corpuscles of the former as an exemplification of those of the latter.

2nd. Hewson, it is known, gave the name of "central particles" to the nuclei of the red corpuscles of the blood of the oviparous Vertebrata. He at the same time supposed that he had seen similar particles in the interior of the red corpuscles of the blood of Man and the Mammifera.

Having observed in the juice of lymphatic glands, bodies which he supposed to be free, and which he recognised to be similar to the central particles of the red corpuscles of the blood, Hewson came to the conclusion that the central particles are at first free, and that they subsequently become surrounded with the red coloured cell-wall, which he called "vesicular portion[1]."

How Hewson should have come to this conclusion is explicable, as regards the juice of the lymphatic glands of Birds, on the following supposition. In the juice of the lymphatic glands of Birds, it has been above seen, that nucleated cells in the uncoloured stage exist in great numbers; but that whilst the nucleus is very distinct, the cell-wall is extremely pale. In consequence of this Hewson appears to have overlooked the cell-wall and distinguished the nucleus alone, but when the cell-wall had already become evident around the nucleus by having acquired colour, he naturally supposed that it was a new formation.

How Hewson should have come to the same conclusion as regards the lymph of Man and the Mammifera, however, is scarcely explicable on any supposition. For to say nothing of the circumstance that there is not a "central particle" in the red corpuscle of the blood of the Mammifera, Hewson's description of "central particles" in the juice of the lymphatic glands of the Mammifera cannot apply to the cellæform nuclei of the uncoloured nucleated cells, seeing that the cellæform nucleus of the uncoloured nucleated cell is nearly, if not as large, as the perfect red corpuscle itself, which he alleged to contain the "central particle." The difficulty is still the same, even supposing Hewson to refer to free cellæform nuclei in the uncoloured stage, and still greater supposing him to refer to granule-cells.

3rd. Since Hewson's time the cell-wall of the uncoloured nucleated cell in the lymph, especially of the oviparous Vertebrata, has continued to be generally overlooked, and the nucleus described as free. The nucleus moreover has been confounded with the granule-cell, which is what is commonly taken as the type of the "lymph-corpuscle." The same confusion prevails in regard to the lymph of Man and the Mammifera, with this additional complication, viz. that granule-cells have been confounded with free cellæform nuclei in the uncoloured stage.

Wagner participating in the error of confounding granule-cells with cellæform nuclei, but having seen and distinguished the cell-wall of nucleated cells, he has taken their cellæform nucleus for a granule-cell, which had acquired a cell-wall around it.

Of all the authors I am acquainted with, Henle alone has accurately distinguished the different forms of the corpuscle of the lymph of Man and the Mammifera, without however pointing out their relationship; but in his account of the corpuscles of the lymph of the Frog, he has fallen into inaccuracies similar to some of those which have been above pointed out.

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[1] He believed, it may be further remarked, though not immediately to our present purpose, that the central particles were first formed in the lymphatic glands, and, after being received into the blood from the trunk of the lymphatic vessels, were in the course of the circulation carried to the spleen, where they acquired the red vesicular portion; and so the "red corpuscle" was formed. He believed also that the addition of the vesicular portion might likewise take place in the lymphatic glands.

## Explanation of the Plate.

## PLATE I.

### Lamprey.

Though it was not considered necessary to describe the blood-corpuscles of the Lamprey particularly, a few figures of them are given for the sake of comparison.

*First Phase.—Granule-cell.*

Fig. 1. Granule-cell somewhat distended by the action of water.

*Second Phase.—Nucleated Cell.*

Fig. 2. A blood-cell in a state of transition from the phase of granule-cell to that of nucleated cell.

Fig. 3. Nucleated cell, uncoloured stage.

Fig. 4 *a*. Nucleated cell, coloured stage.

Fig. 4 *b*. The same cell after the action of acetic acid, whereby the nucleus has been distinctly brought out.

### Skate.

*First Phase.—Granule-cell.*

Fig. 1. A granule-cell in the coarsely granular stage, presenting a clear spot on one side indicating the place of the nucleus.

Fig. 2 *a*. Granule-cell distended by the action of water.

Fig. 2 *b*. The same cell after the action of acetic acid. The granules have been dissolved and a cellæform nucleus brought into view.

Fig. 3. Granule-cell in the act of changing its shape, with movements of its granules, as described, par. 30 *et seq*.

Fig. 4 *a*. Granule-cell in the finely granular stage after the action of water.

*Second Phase.—Nucleated Cell.*

Fig. 4 *b*. A cell in transition from the phase of granule-cell to that of nucleated cell.

Fig. 5 *a*. Nucleated cell, uncoloured stage, circular.

Fig. 5 *b*. A similar cell collapsed.

Fig. 6. Nucleated cell in transition from the circular and uncoloured nucleated cell to the oval red nucleated cell, or nucleated cell in the second or coloured stage.

Fig. 7. The nucleated cell in the second or coloured stage.

### Frog.

*First Phase.—Granule-cell.*

Fig. 1 *a*. Granule-cell in coarsely granular stage, and in the act of changing its shape (par. 59.).

Fig. 1 *b*. The same cell after the action of acetic acid. The granules have been dissolved and a cellæform nucleus exposed.

Fig. 2 *a*. Granule-cell in finely granular stage, after having collapsed and shot out processes.

Fig. 2 *b*. The same, distended by water.

Fig. 2 *c*. The same, after the action of acetic acid. If the acid has been very much diluted, and applied only after the distention of the cell by water, the appearance of the single large cellæform nucleus is produced; in the contrary case, the appearance of several small ones.

*Second Phase.—Nucleated Cell.*

Fig. 2 *d*. Cell in transition from granule to nucleated phase.

Fig. 3. Nucleated cell, uncoloured stage. The very pale cell-wall is seen on one side only of the very distinct nucleus.

Figs. 4 and 5. Nucleated cells, uncoloured stage, the one circular and the other oval.

Fig. 6. Nucleated cell, coloured stage, or the well-known oval "red corpuscle."

COMMON FOWL.

It has not been thought necessary particularly to describe the blood-corpuscles of the Bird. These few figures of the blood-corpuscle of the Common Fowl have been given merely for the sake of reference and comparison.

*First Phase.—Granule-cell.*

Fig. 1. Granule-cell, coarsely granular stage.

Fig. 2. Granule-cell, finely granular stage.

*Second Phase.—Nucleated Cell.*

Fig. 3. Nucleated cell, uncoloured stage.

Fig. 4 *a*. Nucleated cell, coloured stage, but still circular.

Fig. 4 *b*. The same cell, with the nucleus brought into view by the action of acetic acid.

Fig. 5. Nucleated cell, coloured stage, oval.

MAMMIFEROUS EMBRYO—OX, $1\frac{1}{4}$ inch long.

*First Phase.—Granule-cell.*

Fig. 1. Granule-cell, coarsely granular stage.

Fig. 2. Granule-cell in transition from coarsely granular to

Fig. 3. Finely granular stage. This cell, in which the cellæform nucleus is already visible through the finely granular contents, may be viewed as a transition to the phase of nucleated cell.

*Second Phase.—Nucleated Cell.*

Fig. 4. Nucleated cell, uncoloured stage. The cell-wall, which is very pale, is seen all round the very distinct cellæform nucleus.

Fig. 5. Nucleated cell, uncoloured stage. The cell-wall, which has just been brought out by distention with water, is extremely pale, and is seen only on one side of the very distinct cellæform nucleus.

Fig. 6. A nucleated cell in transition to the coloured stage.

Fig. 7 *a*. Nucleated cell, coloured stage, circular.

Fig. 7 *b*. The same, after the action of acetic acid.

Fig. 8. Nucleated cell, coloured stage, oval.

*Third Phase.—Free Cellæform Nucleus.*

Fig. 9. Free cellæform nuclei, coloured stage, or "red corpuscles," similar to those of the fully-formed blood of the animal.

MAN AND THE MAMMIFERA.

*First Phase.—Granule-cell.*

Fig. 1 *a*. The granule-cell of human blood in its coarsely granular stage distended by water.

Fig. 1 *b*. The same, after being acted on by acetic acid. The granules have been dissolved and a single cellæform nucleus disclosed.

Fig. 2 *a*. The granule-cell of human blood in its finely granular stage in a state of collapse.

Fig. 2 *b*. The same, distended by water.

Fig. 3 *a*. The granule-cell, coarsely granular stage, of the blood of the Horse.

Fig. 3 *b*. The same, after being acted on by acetic acid.

Fig. 4. The granule-cell, finely granular stage, of the blood of the Horse.

Fig. 5 *a*. The granule-cell, coarsely granular stage, of the blood of the Elephant.

Fig. 5 *b*. The same, in process of being acted on by acetic acid.

Fig. 6. The granule-cell, finely granular stage, of the blood of the Elephant.

*Second Phase.—Nucleated Cell.*

Fig. 7. Nucleated cell of human blood, uncoloured stage, distended by water.

Fig. 8. The same, in a state of collapse.

Fig. 9. Nucleated cell of the blood of the Elephant in its uncoloured stage.

Fig. 10. The same, in a state of transition from the granule-cell.

Fig. 11. Nucleated cell, uncoloured stage, of the blood of the Goat.

Fig. 12. Nucleated cell, uncoloured stage, of the blood of the Paco.

Fig. 13. Nucleated cell, coloured stage, from the blood of the Horse.

Fig. 14. Nucleated cell, coloured stage, from the blood of the Elephant.

Fig. 15 *a*. Coloured nucleated cell from human blood.

Fig. 15 *b*. Nucleated cell, in transition from uncoloured to coloured stage, from the blood of the Horse.

Fig. 16. Nucleated cell, in transition from uncoloured to coloured stage, from the blood of the Sheep.

Fig. 17. Nucleated cell, in transition from uncoloured to coloured stage, from the blood of the Paco.

*Third Phase.—Free Cellæform Nucleus.*

Fig. 18. Free cellæform nucleus, coloured stage, of human blood.

Fig. 19. Free cellæform nucleus, uncoloured stage, of the blood of the Horse.

Fig. 20. Free cellæform nucleus, coloured stage, of the blood of the Horse.

Fig. 21. Free cellæform nucleus, in transition from uncoloured to coloured stage, of the blood of the Horse.

Fig. 22. Free cellæform nucleus, coloured stage, of the blood of the Sheep.

Fig. 23. Free cellæform nucleus, coloured stage, of the blood of the Elephant.

Figs. 24 and 25. Free cellæform nuclei, coloured stage, of the blood of the Goat.

Fig. 26. Free cellæform nucleus, uncoloured and circular stage, of the blood of the Paco.

Fig. 27. Free cellæform nucleus, coloured and oval stage, of the blood of the Paco.

Figs. 28, 29, 30. Free cellæform nuclei, in transition from uncoloured and circular stage to coloured and oval stage, of the blood of the Paco.

Fig. 31. Free cellæform nucleus, coloured stage, of the blood of the Paco rendered circular by the action of water.

V. *The Blood-corpuscle considered in its different Phases of Development in the Animal Series. Memoir* II.—*Invertebrata.* *By* T. Wharton Jones, *F.R.S., Lecturer on Anatomy, Physiology and Pathology, at the Charing-Cross Hospital, &c.*

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THE invertebrate animals, of which I am here about to consider the blood-corpuscle in its different phases of development, are some of the more readily procurable examples of the divisions Annulosa and Mollusca. The order in which I purpose to proceed is from the higher to the lower classes of each division, an order the contrary of that adopted in the case of the Vertebrata.

### Annulosa.

1. My examples of Annulosa are from the classes of Crustacea, Arachnida, Insecta, and Annelida.

### *Examination of the Blood-corpuscles of Crustacea.*

2. As examples of Crustacea I have taken Crabs and Lobsters.

### *Blood-corpuscles of Crabs.*

3. By snipping off the end of one of the legs of a Crab, the blood flows freely out from the stump, and is thus readily obtained*. A small quantity of the blood being received on a plate of glass direct from the animal, and forthwith examined under the microscope, it is seen to contain a very considerable number of corpuscles.

4. *Kinds of Corpuscles.*—Both granule-cells and nucleated cells,—the latter the more numerous,—may be distinctly recognised.

5. *Shape of the Cells.*—If very great expedition has been employed in receiving the blood as it flows from the animal on the plate of glass, spreading it out and transferring it to the microscope for examination, the granule-cells may be seen to be of an elongated oval shape, the nucleated cells spindle-shaped (figs. 1 and 6.). These shapes however are speedily changed.

6. As regards the granule-cell, it tends to become circular (figs. 2 and 3.), but it

* The blood as it flows from the animal appears of a pale reddish gray or neutral tint; when collected in a watch-glass, it separates into a spongy-looking mass and a serous fluid. This spongy-looking mass, which is of a slight pink colour, consists principally of the corpuscles aggregated, there being little spontaneously coagulable material in the plasma to form a true clot. The serous fluid, which is bluish by reflected light, and reddish by transmitted light, is coagulated by heat, and also by *acetic acid*.

is also soon observed that its cell-wall bulges out here and there into round processes which again subside, whilst another part of the cell-wall bulges out in the same way. This change of shape, it will be perceived, is similar to that which I particularly described in the case of the granule-cell of the blood of the Skate, only it is to be remarked, that it is not accompanied by such a well-marked movement of the contained granules. Besides bulging out into round processes, the cell-wall may be seen to shoot out into cilia-like processes also (fig. 4.).

7. As regards the nucleated cell, it also first tends to become circular, and then shoots out its wall into processes which are usually more cilia-like than in the case of the granule-cell; and being in all directions like radii, the cell comes to present a stellate appearance (figs. 7, 8, 9.). Sometimes a cell is seen to shoot out into processes in two principal directions only, these processes again shooting out into smaller, so that the cell acquires a caudate form. Between this shape and the former there is every intermediate degree.

8. *Size of the Cells.*—When first examined, and before their shape has become changed, the granule-cells are about $\frac{1}{1800}$th of an inch long by about $\frac{1}{3000}$th of an inch broad. The nucleated cell is rather less in size, or perhaps with about the same length; it is not quite so broad.

9. *Structure of the Granule-cell.*—There are both coarsely and finely granular stages of the granule-cell. The granules appear more or less fused together, and are of a slightly greenish yellow colour when the microscopical examination is made by day-light.

10. By the addition of water the cell-wall is brought out very distinctly. Imbibing the water, the cell becomes distended and acquires a circular shape, and this as well after it has shot out into processes as when the water is added to the blood just drawn, and before the cell has lost its original shape. Fig. 5 *a*. represents a granule-cell as acted on by water.

11. In consequence of acetic acid producing a copious white curdy precipitate on its addition to the blood, some difficulty was at first experienced in studying the action of that acid on the blood-cells, but by previously diluting the blood with much water, the precipitate was not considerable enough to prevent the observation of the action of the acetic acid on the blood-cells.

12. When the acid comes into contact with the granule-cell, the granulous mass is dissolved, the granules disappearing one after the other, as already described in regard to the granule-cell of the Skate, &c. After the solution of the granulous mass, there is discovered in the interior of the cell, a cellæform nucleus about $\frac{1}{7000}$th of an inch in diameter (fig. 5 *b*.).

13. A clear spot which may be observed on the side of the granule-cell in blood just drawn (figs. 1, 2, 3.), and which is similar to that presented by the granule-cell of the blood of the Skate, appears to be, as in the Skate, produced by the nucleus peering through a pore in the granulous mass.

14. *Structure of the Nucleated Cell.*—By the action of water, the nucleated cell is distended and rendered circular even after it has shot out processes and acquired a stellate appearance. During this action of the water, the processes of the cell-wall may be observed to become broader and shorter, and to run into each other until the outline of the whole cell is circular, as in fig. 10. Within the cell thus distended, the cellæform nucleus is seen circular, and about $\frac{1}{3500}$th of an inch in diameter.

15. By the addition of dilute acetic acid, the nucleated cell is not further materially affected.

16. Although a decided coloured stage of nucleated cell does not exist in the blood of the Crab, it is proper to observe that many of the nucleated cells transmit the light through their interior of a very slight red tint. The circumferential doubling of the cell-wall transmitting the light quite colourless, forms with the interior a well-marked contrast. Though the red tint is more marked by lamp-light, it is sufficiently distinct by day-light. It has been above mentioned, that when the corpuscles collected in a mass are viewed by reflected light, they also appear slightly pink.

17. Besides granule-cells and nucleated cells such as have now been described, cells occur in which the cellæform nucleus is already visible, but still surrounded by some granulous matter (fig. 11.). These may be viewed as cells in a state of transition from granule to nucleated cell.

18. It may thus be concluded, that in Crabs the blood-corpuscle presents itself in two different phases of development, as in the oviparous Vertebrata, viz. the phases of granule-cell and of nucleated cell, the granule-cell being the first phase of development, the nucleated cell the second phase.

19. Floating about amongst the blood-corpuscles, there are seen a few elementary granules, the larger of which, from $\frac{1}{6000}$th to $\frac{1}{12.000}$th of an inch in diameter, have the form of biconcave circular discs.

### *Blood-corpuscles of the Lobster.*

20. The description which has now been given of the blood-corpuscles of the Crab, is in all respects applicable to those of the Lobster*.

### *Examination of the Blood-corpuscles of Arachnida.*

21. It was the *common Spider* the blood of which I examined; and the mode in

* The changes of form which the blood-corpuscles of the Lobster, like those of the Crab, undergo after the blood is drawn, were described and delineated by Hewson as accurately as his microscope appears to have enabled him to observe them. Hewson also mentions what I have found to be the case, that the blood of the Lobster, "after being some time exposed to the air, jellies, but less firmly than the blood of more perfect animals." In this respect the blood of the Lobster differs from the blood of the Crabs which I examined; the blood of the latter, as I have above stated, not containing a sufficient quantity of spontaneously coagulable material to form a perfect clot. It may be proper to remark that it was in the beginning of winter when I examined the Crab's blood, and in the beginning of spring when I examined that of the Lobster.

which I obtained the blood was the same as that already stated in regard to the Crab and Lobster, viz. by snipping off one of the legs and receiving on a plate of glass the minute drop of blood which oozed out.

22. The blood-corpuscles of the Spider are almost identical with those of the Crab and Lobster. There are both granule-cells and nucleated cells,—the latter the more numerous,—which when examined immediately on the blood being drawn from the animal, present, the former an oval, the latter an elliptical shape (figs. 1, 2 and 7.). These shapes, however, are in general soon lost. The cells become roundish, their cell-wall at the same time shooting out into processes. This shooting-out of the cell-wall into processes is more especially presented by the granule-cells (fig. 3.), for in the case of the nucleated cell, it might be said that the cell-wall in general rather becomes shrivelled and collapsed than shoots into cilia-like processes (fig. 8.).

23. The size of the corpuscles is much the same as in the Crab.

24. There are both coarsely and finely granular stages of the granule-cell (figs. 1 and 2.), and also cells in transition from the granule to the nucleated phase.

25. By the action of water, the cell-wall of both granule and nucleated cell is distended and brought out in the same way as in the case of the blood-cells of the Crab (figs. 4, 5 and 9.), and by the action of acetic acid the granules of the granule-cells are broken up and dissolved, leaving the nucleus exposed (fig. 6.).

26. In regard to a coloured stage of nucleated cell, what was above said in the case of the Crab (par. 16.), is applicable here.

27. As in the blood of the Crab also, a few elementary granules are seen floating about.

*Examination of the Blood-corpuscles of Insecta.*

28. In entering on the examination of the blood-corpuscles of insects, the advantage of having first examined those of Crabs and Spiders is strongly felt; for the blood of these animals being readily and certainly obtainable free from admixture with foreign particles, which might be confounded with blood-corpuscles, the characters of their blood-corpuscles will serve as a guide by which to recognise their analogues in the blood of insects and other invertebrate animals, the conformation and structure of the bodies of which are such that we cannot be sure that the blood obtained from them is quite free from foreign admixture.

29. As examples of perfect insects I have taken Beetles, and as examples of chrysalises I have taken those of the Cabbage Butterfly*.

*Blood-corpuscles of Beetles.*

30. The matter examined as the blood of these insects was the clear greenish

* I have also examined the blood-corpuscles of the Caterpillar, and found them in essential particulars the same. Having, however, as yet obtained but one specimen of a caterpillar of the Cabbage Butterfly, I am not able to give here a description and delineation of the blood-corpuscles. This, however, I shall be able to do on another occasion.

yellow fluid which exuded when a small wound was made in the body of the animal.

31. In this fluid there were recognised, amidst a number of other corpuscles the nature of which will be inquired into below, corpuscles corresponding in essential characters to the granule and nucleated blood-cells of the Crab and Spider.

32. *Granule Blood-cells.*—These were, when seen immediately on the blood being drawn, of an elliptical or oval shape (fig. 1.), but they soon became round. In this state some might be seen with the cell-wall shot out into small cilia-like processes (fig. 2.). By the action of water, the cell becoming distended, these processes disappeared (fig. 3.).

33. The size of the cells under consideration was on an average $\frac{1}{1000}$th of an inch long by $\frac{1}{4000}$th broad.

34. The cells were most of them pale-looking, but some presented coarser and more refracting granules than others.

35. *Nucleated Blood-cells.*—These were much more numerous than the granule blood-cells. When the blood was first examined on being drawn, the cellæform nucleus, of an oval shape, measuring $\frac{1}{3000}$th by about $\frac{1}{7000}$th of an inch, and reddish in its interior, was the part most distinctly seen. The cell-wall, which might sometimes be seen of an elliptical shape (fig. 4.), was in general already found shrivelled and collapsed around the cellæform nucleus, or shot out into processes (fig. 5.). It was extremely pale and not always very readily distinguished. On the addition of water, however, it became distended and was then pretty distinctly seen (fig. 6.). In this state it was circular and measured about $\frac{1}{1000}$th of an inch in diameter.

36. In regard to the other kinds of corpuscles contained in the blood of the Beetle, they were, 1st,—a few corpuscles resembling the cellæform nuclei of the nucleated cells, but around which no cell-wall could be detected; 2nd, some oil-globules; 3rd, a great number of elementary granules in size from about $\frac{1}{7000}$th of an inch in diameter downwards, the larger being biconcave, circular, and of a yellowish or reddish colour.

### *Blood-corpuscles of the Chrysalis of the Cabbage Butterfly.*

37. The matter examined as blood was the clear green fluid which flowed out on slight pressure, when a small point of the anterior end of the chrysalis was snipped off.

38. In this fluid there were both granule-cells and nucleated cells (figs. 7, 8, 9, 10, 11, 12.), together with elementary granules.

### *Examination of the Blood-corpuscles of Annelida.*

39. As examples of Annelida, I have taken the common Earth-worm and the medicinal Leech.

*Blood-corpuscles of the Earth-worm.*

40. The blood was most readily obtained for examination from the abdominal vessel, but in abstracting it, care was required to guard against its becoming mixed with the secretion poured out from the skin in great abundance when the animal was wounded.

41. The corpuscles of the blood of the Earth-worm are remarkable for their great size, being on an average $\frac{1}{1100}$th or $\frac{1}{1800}$th of an inch in diameter. There are both granule and nucleated cells.

42. *Granule Blood-cells.*—Of these there are both coarsely and finely granular stages. I have not observed the cell-wall of the granule-cell in the coarsely granular stage to shoot out into more than perhaps a single bud-like process (fig. 1.), but in the finely granular stage, the cell-wall shoots out into so many cilia-like processes, that the cell soon after the blood is drawn presents a stellate form (fig. 2.), the granulous mass about $\frac{1}{1800}$th of an inch in diameter occupying the centre.

43. By the action of water the granule-cells become uniformly distended (figs. 3 and 4.), the stellate form into which the cell in the finely granular stage had fallen disappearing. In this state of distention the cells measure $\frac{1}{1100}$th or $\frac{1}{1800}$th of an inch in diameter, but their granulous contents do not fill their whole interior.

44. Acetic acid causes the granules of the cell in the coarsely granular stage to break up, but does not dissolve their substance. The granulous contents of the cell in the finely granular stage it renders more transparent.

45. *Nucleated Blood-cells.*—Instead of perfect nucleated cells, the cells about to be described under this head might, perhaps, rather be said to be cells in transition from the granule-cell phase, inasmuch as the nucleus, though quite evident, is still surrounded by granular matter.

46. The cells under consideration have always been collapsed when first seen, though the blood was examined as quickly as possible after being drawn; but I have observed them in the act of shooting out their cell-wall into processes (fig. 5.), like the granule-cell in the finely granular stage.

47. By the action of water, the processes are made to disappear by the cell becoming uniformly distended. In this state the cell measures about $\frac{1}{1100}$th of an inch in diameter, and in its interior is seen the cellæform nucleus about $\frac{1}{3000}$th of an inch in diameter, with a finely granulous mass surrounding it (fig. 6.). This granulous mass is not dissolved entirely by acetic acid, but it is rendered more transparent.

48. The red colour of the blood of the Earth-worm is, as is known, seated in the plasma, but it is to be remarked that some of the nucleated cells appear very slightly tinged, as also the nuclei, in their interior.

49. Besides the nucleated cells now described, corpuscles are met with altogether like their nucleus and its surrounding granulous mass, both in form and size.

50. Lastly, there are a few corpuscles like perfectly free nuclei, and a considerable number of elementary granules, the larger, of the form of biconcave circular discs.

*Blood-corpuscles of the Medicinal Leech.*

51. The blood examined was obtained by making incisions on the back or side of the animal after having carefully wiped the surface of the skin. In making these incisions care was taken not to cut into the stomach, or its cæcal appendages, in order to obviate the possibility of foreign blood which might have been taken in as food, becoming mixed with the real blood of the animal itself.

52. In the first place it is worthy of remark, that whilst the corpuscles of the blood of the Earth-worm are the largest which I have yet found in any invertebrate animal, the corpuscles of the blood of the Leech are the smallest.

53. When the blood of the Leech is examined under the microscope as soon after its abstraction from the animal as possible, numerous corpuscles are seen having the appearance of very pale, shining, colourless, fusiform filaments, about $\frac{1}{5000}$th of an inch in length, and about $\frac{1}{7000}$th of an inch in breadth, less or more (fig. 9.), suspended in the red-coloured plasma. Very soon, however, the corpuscles are seen gradually to become shorter and somewhat broader, until at last they acquire an irregular circular form (fig. 10.).

54. In this state the majority of the corpuscles appear to be composed of a nucleus surrounded by a collapsed and shrivelled cell-wall. And that this is so, is reduced to a certainty by the addition of water, which causes the cell to become distended and to acquire a circular form, whilst the outline of the cellæform nucleus appears more distinctly defined (figs. 11 and 12.).

55. Thus distended, the cell is from $\frac{1}{3000}$th to $\frac{1}{3600}$th of an inch in diameter, and the nucleus $\frac{1}{7000}$th, or more.

56. Many of the cells transmit the light slightly tinged red, as if there was some colouring matter lining their interior (fig. 12.).

57. The corpuscles of the blood of the Leech now described, it will have been observed, are *nucleated cells*.

58. *Granule-cells.*—Corpuscles in the blood of the Leech referrible to this head are few in number. Examples of them are delineated in figs. 7 and 8*.

* I subjoin here a description of the corpuscles of the blood vomited by leeches which have never been used. This blood I found to be composed of a coloured plasma, and numerous corpuscles having a considerable resemblance to those of the blood of the animal itself after having lost their original fusiform shape, and become somewhat distended by water. On the whole, however, the corpuscles of the blood in question were somewhat larger, and appeared perhaps better defined than those of the Leech itself, especially the granule-cells, which were also more numerous, though still not so numerous as the nucleated cells.

Of the granule-cell there were both coarsely and finely granular stages (figs. 13 and 14.). There were cells in transition from the granule to the nucleated phase (fig. 15.). The nucleated cells (figs. 16, 17 and 18.) were most of them circular, but some were oval. Most appeared tinged red in their interior, those which did not had their cell-wall so very pale that it was apt to be overlooked.

The blood contained in the stomach of leeches which have not been used, and of which I have now briefly described the corpuscles, from what animal is it derived? The resemblance of the blood to that of the animal itself suggests the probability that medicinal leeches suck the blood of some other kind of leech. That they suck each other's blood has been positively denied by Dr. Rawlins Johnson.

## Mollusca.

59. My examples of Mollusca are confined to the classes of Gasteropoda and Acephala,—from the former I have taken the Whelk (*Buccinum magnum*),—from the latter the Mussel (*Mytilus edulis*), in which to examine the blood-corpuscles.

### *Blood-corpuscles of the Whelk.*

60. The shell having been broken to pieces from around the animal, the blood was readily obtained from the great vessels of the heart, or from the heart itself; but notwithstanding every care in abstracting it, the blood was not always quite free from admixture with foreign particles, such as ciliated epithelium-cells and the like.

61. *Kinds of Corpuscles.*—There were granule-cells and nucleated cells essentially similar to those of the blood of Annulosa.

62. *Granule-cells.*—By the time the blood could be examined, the granule-cells had, for the most part, become agglomerated together in groups, and their cell-wall was seen already shot out into processes (fig. 1.).

63. Both coarsely and finely granular stages of the granule-cell were to be recognised.

64. The size of the granule-cell was on an average from $\frac{1}{3000}$th to $\frac{1}{3000}$th of an inch.

65. By the action of water the cell became uniformly distended (fig. 2.), to the size of about as much as $\frac{1}{1500}$th of an inch in diameter and then burst, the cell-wall disappearing and leaving the granulous contents in a mass with the nucleus visible in the centre (fig. 3.).

66. *Nucleated cells.*—Some of these were seen with the cell-wall shot out into processes (fig. 4.); some not (figs. 5, 6 and 7.).

67. Those cells of which the cell-wall did not shoot out into processes, were for the most part circular (figs. 5 and 6.); but some also occurred which appeared to be elliptical (fig. 7.), though these might have been circular cells seen somewhat raised up on edge. The cells under consideration often presented a finely granulous matter in their interior, and some were slightly tinged of a red colour. Their size was about $\frac{1}{3500}$th of an inch in diameter.

68. Those cells of which the cell-wall shot out into processes, when uniformly distended by water, were larger, measuring about $\frac{1}{1650}$th of an inch in diameter (fig. 4 *a*.).

69. The nucleus, oval or circular, and reddish in the interior, measured on an average from $\frac{1}{4800}$th to $\frac{1}{3650}$th of an inch, these being the means when the nucleus was oval.

70. Besides the corpuscles now described, there were a few like free nuclei, and a great quantity of elementary granules of different sizes, the larger biconcave and circular.

*Blood-corpuscles of the Mussel.*

71. The way in which I obtained the blood of the Mussel was this:—I first removed one valve of the shell with as little injury to the animal as possible, and allowed the contained fluid to drain away; I then opened the great vessel proceeding from the heart to the anterior part of the body, and introducing the microscopical forceps towards the heart, took up enough of the blood for one examination. Notwithstanding this careful procedure, the blood was often mixed with foreign corpuscles, such as the spermatozoa and ova of the animal.

72. *Kinds of Blood-corpuscles.*—There were in the first place *granule-cells*, which, like those of the blood of the Whelk, were found by the time the blood could be examined, already for the most part agglomerated together, and presenting their cell-wall shot out into processes (figs. 1 and 2.). The granulous contents appeared as if fused, forming a clear, more or less strongly refracting mass.

73. By the action of water this mass was broken up into separate granules, and at the same time the cell was observed to become gradually distended and rendered uniformly circular by the undoing of the processes into which its wall had shot. This undoing of the processes I have often, as in the case of the blood-cells of other animals, watched step by step.

74. When the cell had become fully distended, the granulous contents were usually next seen to escape by the bursting of the cell-wall, and to accumulate around its outside, whilst a nucleus from about $\frac{1}{7000}$th to $\frac{1}{4000}$th of an inch in diameter, and now for the first time seen, remained in the interior; the cell-wall, different from that of the granule-cell of the Whelk, continuing quite visible and otherwise entire (fig. 3.).

75. Such was the condition in which the granule-cells presented themselves in the blood of fresh mussels. In the blood of mussels which had been kept for some days in the house without food or change of water, but still alive though weak, the granule-cells were found, for the most part, to continue uniformly distended after the blood was drawn, their cell-wall not shooting out into processes (figs. 4 and 5.). The contents appeared at the same time more broken up into granules. In this state the size of the cells was from $\frac{1}{1800}$th to $\frac{1}{3000}$th of an inch in diameter. They appeared in both coarsely and finely granular stages.

76. Cells in which the nucleus is already distinctly visible without the application of any reagent, were few in number in the blood of perfectly fresh mussels, but in the blood of mussels which had been kept some days, they presented themselves in considerable numbers (figs. 9, 10, 11 and 12.).

77. The corpuscles in which a nucleus had thus become distinctly visible were, as they occurred in the blood of fresh mussels, distinguished by the following characters:—Circular but sometimes ovoid, somewhat prominent in the middle, with a uniform and strongly marked contour, of an opaline aspect, and measuring on an average about $\frac{1}{3400}$th of an inch in diameter, many being both smaller and larger (figs. 6 and 7.).

78. By the action of water the contents of these corpuscles, previously homogeneous looking, adherent to the cell-wall, and concealing the nucleus, became broken up into granules, so that the cell-wall and a nucleus came more or less distinctly into view, with characters similar to those presented by them after the animals had been kept some time as above mentioned, and which I now proceed to describe (fig. 8.).

79. The cell was generally circular but sometimes oval, and its wall did not shoot out into processes. It measured on an average $\frac{1}{3400}$th of an inch, some more, some less.

80. The nucleus was circular or oval. In the former case it measured about $\frac{1}{3000}$th of an inch in diameter, in the latter, the mean of the two diameters was on an average the same as this. Some were much elongated but narrow, others short but broad.

81. The circular cells had the nucleus either circular or oval, but when the cell was oval the nucleus was oval also, and usually elongated and narrow. Sometimes the oval nucleus appeared almost free, the cell-wall very much contracted, being attached to one side only.

82. In addition to the corpuscles now described, there were observed corpuscles very pale and few in number, apparently the nuclei of the nucleated cells become free and in process of disappearing.

83. Lastly, there was a great number of elementary granules; the larger, of the form of biconcave circular discs, measuring on an average $\frac{1}{6000}$th or $\frac{1}{7000}$th of an inch in diameter, less or more.

84. It thus results that in all the invertebrate animals in which the blood has now been examined, the blood-corpuscle presents itself in the phases of granule-cell and of nucleated cell; that in the phase of granule-cell it occurs in more or less well-marked coarsely and finely granular stages; that in the phase of nucleated cell it occurs in the uncoloured stage; but that as regards a coloured stage, though such cannot be said to be altogether wanting in some of the animals, the coloration of the cell is in a very slight degree. The cellæform nucleus is found more frequently and more decidedly coloured.

85. It also results that corpuscles exist in transition from the phase of granule-cell to that of nucleated cell.

86. It farther results that corpuscles occur in the blood of some of the animals, which appear to be the nuclei of the nucleated cells become free. Such, however, are to be viewed as abortions rather than as examples of a third phase, or phase of free cellæform nucleus.

87. Lastly, it results that corpuscles are met with in greater or less numbers belonging to the category of the lowest forms of organic elements, viz. elementary granules.

EXPLANATION OF THE PLATE.

## PLATE II.

### ANNULOSA.

CRUSTACEA.—CRABS.

*First Phase.—Granule-cell.*

Fig. 1. Granule-cell as it appears when the blood is examined immediately on being drawn.

Figs. 2 and 3. Granule-cells changed in shape as they appear very soon after the blood has been drawn.

Fig. 4. Granule-cell, with the cell-wall in view at the circumference, and shooting out bud-like processes here and there.

Fig. 5 *a*. Granule-cell after being acted on by water.

Fig. 5 *b*. Granule-cell after being acted on by acetic acid. The granules have all disappeared by solution, and a small cellæform nucleus has thus come into view.

*Second Phase.—Nucleated Cell.*

Fig. 6. Nucleated cell as it appears when the blood is examined immediately on being drawn.

Figs. 7, 8, 9. The nucleated cell in different degrees of change of shape.

Fig. 10. Nucleated cell after being acted on by water.

Fig. 11. Another, after being treated in the same way, but still presenting some remains of the granules of the first phase, and a smaller cellæform nucleus than the preceding.

ARACHNIDA.—SPIDER.

*First Phase.*

Fig. 1. Granule-cell, coarsely granular stage.

Fig. 2. Granule-cell, finely granular stage; both as seen when the blood was examined immediately on being drawn.

Fig. 3. Granule-cell changed in shape, and with the cell-wall shot out into processes.

Figs. 4 and 5. Granule-cells in coarsely and finely granular stages distended by water.

Fig. 6. Granule-cell after the granules have been dissolved by acetic acid.

*Second Phase.*

Fig. 7. Nucleated cell as it appeared when the blood was examined immediately on being drawn.

Fig. 8. The same, changed in shape, as it appeared soon after the blood was drawn.

Fig. 9. The same, distended by water.

INSECTA.—BEETLE.

*First Phase.*

Fig. 1. Granule-cell as it appeared when the blood was examined as soon as possible after being drawn.

Fig. 2. The same, changed in shape—the cell-wall shot out into processes—as it appeared soon after the blood was drawn.

Fig. 3. The same, after the action of water.

*Second Phase.*

Fig. 4. Nucleated cell, as it may sometimes be seen when the blood is examined immediately on being drawn.

Fig. 5. The same cell, changed in shape. The cell-wall extremely pale, shrivelled, and shot out here and there into processes.

Fig. 6. The same, distended by water.

INSECTA.—CHRYSALIS OF CABBAGE-BUTTERFLY.

*First Phase.*

Figs. 7 and 8. Granule-cells in coarsely and finely granular stages.

Fig. 9. Granule-cell, with the cell-wall shot out into processes.

*Second Phase.*

Fig. 10. Nucleated cell of its original shape.

Fig. 11. The same, with the cell-wall shrivelled and shot out into processes.

Fig. 12. The same, distended by water.

ANNELIDA.—EARTH-WORM.

*First Phase.*

Fig. 1. Granule-cell, coarsely granular stage. The cell-wall shot out into a single bud-like process.

Fig. 2. Granule-cell, finely granular stage. The cell-wall shot out into cilia-like processes.

Figs. 3 and 4. The same cells distended by water.

*Second Phase.*

Fig. 5. Nucleated cell with its cell-wall shot out into processes.

Fig. 6. The same, distended by water.

ANNELIDA.—MEDICINAL LEECH.

*First Phase.*

Figs. 7 and 8. Granule-cells.

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*Second Phase.*

Fig. 9. Nucleated cell as seen when the blood is examined immediately on being drawn.

Fig. 10. The same, changed in shape, as it appeared soon after the blood is drawn.

Figs. 11 and 12. Nucleated cells distended by water.

*Blood vomited by Medicinal Leech.*

Figs. 13 and 14. Granule-cells, coarsely and finely granular stages.

Fig. 15. A transition cell.

Figs. 16, 17 and 18. Nucleated cells.

## MOLLUSCA.

### Gasteropoda.—Whelk.

*First Phase.*

Fig. 1. Granule-cell, with the cell-wall shot out into processes.

Fig. 2. The same, distended by water.

Fig. 3. The contents and nucleus of the same, set free by the bursting of the cell-wall.

*Second Phase.*

Fig. 4. Nucleated cell, with the cell-wall shot out into processes.

Fig. 4 *a*. The same, distended by water.

Figs. 5, 6 and 7. Nucleated cells, the cell-wall of which did not shoot out into processes.

### Acephala.—Mussel.

*First Phase.*

Figs. 1 and 2. Granule-cells, coarsely and finely granular stages, with the cell-wall shot out into processes.

Fig. 3. Granule cell after the action of water.

Figs. 4 and 5. Granule-cells from a mussel which had been kept some time in the house.

*Second Phase.*

Figs. 6 and 7. Cells from the blood of fresh mussels, by the action of water on which a nucleus has been rendered visible, as in fig. 8.

Figs. 9, 10, 11, 12. Nucleated cells found in considerable number in the blood of mussels kept some time, but few in number in fresh mussels.

# VI. *The Blood-corpuscle considered in its different Phases of Development in the Animal Series. Memoir* III.—*Comparison between the Blood-corpuscle of the Vertebrata and that of the Invertebrata.*

*By* T. Wharton Jones, *F.R.S., Lecturer on Anatomy, Physiology and Pathology, at the Charing-Cross Hospital, &c.*

Received May 7,—Read June 19, 1845.

1. In instituting a comparison between the blood-corpuscle, in its different phases of development, of the Vertebrata and that of the Invertebrata, it is obvious that the examples first taken for the purpose ought to be selected from the lowest class of the oviparous Vertebrata on the one hand, and from the highest class of the highest division of the Invertebrata on the other.

2. In accordance with this I proceed to compare together the blood-corpuscles of the Skate and Crab.

*Comparison between the Blood-corpuscle of the Skate and that of the Crab.*

3. From the observations above related, it results that the blood-corpuscle of the Crab resembles that of the Skate in presenting two different phases of development, viz. the phase of granule-cell and the phase of nucleated cell, and that in these two phases respectively the essential points of structure are the same.

4. The blood-corpuscle of the two animals, however, differs in the degree of development which it attains in the phase of nucleated cell. In the Crab its development is arrested at the uncoloured stage, or at the most at the commencement of the coloured stage. But this absence of nucleated cells in the decided coloured stage in the blood of the Crab, it has been seen is a peculiarity which there is reason to believe is presented by one at least from among oviparous vertebrate animals, viz. *Branchiostoma lubricum*.

5. The blood of this fish may therefore be admitted as probably differing but little in the character of its corpuscles from the blood of the Crab, and as constituting in this respect a transition from the Vertebrata to the Invertebrata.

6. Having thus compared the blood-corpuscle of an animal from the lowest class of oviparous Vertebrata with that of one from the highest class of the highest division of the Invertebrata, we are prepared to institute a comparison between the blood-corpuscle of the oviparous Vertebrata generally, and that of the Invertebrata from Crabs to Mussels.

*Comparison between the Blood-corpuscle of the oviparous Vertebrata generally, and that of Invertebrata.*

7. In the oviparous Vertebrata, from the Skate upwards, it has been seen that the blood-corpuscle in its different phases of development is essentially similar to that of the Skate. In the Invertebrata, from the Crab down as far as we have gone, it has also been seen that the blood-corpuscle in its different phases of development is essentially similar to that of the Crab. The only difference therefore in essential respects between the blood-corpuscle of the oviparous Vertebrata generally and that of the Invertebrata, is the same as that between the blood-corpuscle of the Skate and Crab, viz. that in the phase of nucleated cell, the latter does not attain to a decidedly coloured stage.

8. Hitherto I have altogether excluded the blood-corpuscle of the Mammifera from comparison with that of the Invertebrata; but such comparison I now proceed to make.

*Comparison between the Blood-corpuscle of Mammifera and that of Invertebrata.*

9. In the phase of granule-cell, the blood-corpuscle of the Mammifera and that of Invertebrata resemble each other in essential points of structure. In the phase of nucleated cell they also agree in the absence of a decided coloured stage; in this respect differing from the blood-corpuscle of the oviparous Vertebrata generally and of the early mammiferous embryo.

10. But here the resemblance between the blood-corpuscle of the Mammifera and that of the Invertebrata ceases. In common with the blood-corpuscle of the oviparous Vertebrata, that of the Invertebrata differs from the blood-corpuscle of the Mammifera in not attaining to a decided third phase. The free cellæform nuclei which appear to exist in the blood of some of the Invertebrata, it has been above stated, can only be considered as abortions of such a phase*.

* From neglecting the precautions above observed in instituting a comparison between the blood-corpuscles of the Vertebrata and those of the Invertebrata, Mr. Newport, in a communication recently made to the Royal Society, appears to me to have fallen into a very serious error, as to what corpuscles in the blood of insects and other Invertebrata are analogous to the "red blood-corpuscles" of the Vertebrata.

Overlooking altogether even the well-known differences between the "red blood-corpuscles" of the Mammifera and those of the oviparous Vertebrata, he assumes the "red blood-corpuscle" of the Mammifera as a standard by which to recognise in the blood of insects and other Invertebrata, any corpuscles which may be the analogues of the "red blood-corpuscles" of the Vertebrata generally.

Accordingly, finding in the blood of insects the elementary granules described in Memoir II. of this series of papers, pars. 19, 27, 36, 38, &c., and not being acquainted with the true nature of such particles, he has been misled by their similarity to his assumed type of "red blood-corpuscles," especially in respect of outward form, which is that of biconcave circular discs, into the conclusion that they are the analogues of the "red blood-corpuscles" of the Vertebrata.

The corpuscles in the blood of insects and other Invertebrata which, though little or not at all coloured, I have

11. It was at one time supposed that the red colour of the "red blood-corpuscles" of the Vertebrata is dependent on the iron which they contain, but latterly chemists have come to the conclusion, that though iron is essential to the "red corpuscles," it is not so to their colouring matter.

12. In reference to this question, it occurred to me that it would be interesting to ascertain whether the corpuscles of the blood of an invertebrate animal, notwithstanding their slight degree or total want of colour, contain iron.

13. Accordingly I collected some of the corpuscles of the blood of the Crab, which it is easy to do in consequence of the mode in which they collect together after the blood is drawn, as described in Memoir II. of this series, and in consequence of the plasma not coagulating. These I dried carefully and sent to my friend Professor GRAHAM, who was so kind as to undertake to test them for iron.

14. The annexed letter, from Mr. GRAHAM, will show that the blood-corpuscles of the Crab, though but slightly coloured, do contain iron.

"DEAR JONES,

"I should have communicated sooner the result of the examination of the white corpuscles of the blood of the Crab, of which you sent a small quantity in a dried state, amounting to two or three tenths of a grain. It is that they contain a sensible quantity of iron, perhaps as much as red corpuscles.

"Yours, dear Sir,
"Most truly,
"THOMAS GRAHAM."

"*University College, Nov.* 29, 1844."

15. Of course the presence of iron in the corpuscles of the blood of the Crab is not alone a proof that the red colour of the "red corpuscles" of the blood of the Vertebrata is not owing to iron; it is merely a proof that iron in equal quantity may exist in corpuscles without imparting to them a decided red colour, such as is presented by the "red corpuscles" of the blood of the Vertebrata*.

### *Comparison of the Corpuscles of the Blood of the Invertebrata with the Corpuscles of the Lymph of the Vertebrata.*

16. In the very respect in which the blood of the Invertebrata differs from that of the oviparous Vertebrata generally, viz. the absence of nucleated cells in the decidedly coloured stage, it resembles their lymph† (Memoir I., pars. 101 to 105.).

shown to be the true analogues of the "red blood-corpuscles" of the *oviparous* Vertebrata, viz. the nucleated cells, Mr. NEWPORT considers to be an early stage of the corpuscles which I have described in the same blood under the name of granule-cells, the former, according to him, becoming the latter by the development and accumulation of granules in their interior.

When thus filled with granules, the cells, he appears to believe, burst, and the granules, thus set free, become the alleged analogues of the "red corpuscles" of the Vertebrata.

* The coloured plasma of the blood of the Crab gave unquestionable indications of iron.

† This conclusion, it is to be particularly observed, is to be distinguished from a view first promulgated by

To complete the history of the blood-corpuscle, it remains to inquire into the first formation of the granule blood-cell on the one hand, and into the ultimate fate of the nucleated blood-cell of the Invertebrata, and of the oviparous Vertebrata, and of the free cellæform nucleus of Man and the Mammifera on the other; and also to collate the views given of the development of the blood-corpuscle with the theory of cell development in general.

In executing this task I shall have an opportunity of considering several questions which have suggested themselves in the present series of papers, but which it would have been premature to have entered upon.

Professor Rudolph Wagner, viz. that the corpuscles of the circulating fluid of the Invertebrata are not any of them analogous to the red blood-corpuscles of the Vertebrata, but are all of them analogous to the corpuscles of the lymph, or chyle merely, and that therefore the circulating fluid of the Invertebrata is "mere chyle, not proper blood," proper blood being, as he defines it, "a red coloured fluid containing characteristic corpuscles."

This view appears to me to be distinction without essential difference, and to have arisen,—1st, from an imperfect acquaintance with the corpuscles of the lymph of the Vertebrata, and their relationship to those of the blood of the same division of animals on the one hand, and with the corpuscles of the circulating fluid of the Invertebrata on the other; 2ndly, from laying too much weight on the presence or absence of colour as a distinctive character; thus making no distinction between the "red corpuscle" of the blood of the Mammifera, and the "red corpuscle" of the blood of the oviparous Vertebrata in consequence of their similarity in colour, though there is between them an essential organic difference; whilst between the "red corpuscle" of the blood of the oviparous Vertebrata and the nucleated cell of the circulating fluid of the Invertebrata, in consequence of a supposed total absence of colour in the latter, a decided distinction is made, though there is, in fact, between them no essential organic difference.

Waiving, however, the organic difference between the "red corpuscle" of the Mammifera and the "red corpuscle" of the oviparous Vertebrata, and the organic resemblance between the "red corpuscle" of the oviparous Vertebrata, and the little or not at all coloured nucleated cell of the circulating fluid of the Invertebrata, I would observe in regard to the presence or absence of colour, that nothing bearing on the question can be inferred from it, even in a physiological point of view, seeing that the nature and use of the red colouring matter of the "red blood-corpuscles" are as yet too little known.

The colour of "red blood-corpuscles" is now acknowledged not to be owing to the iron entering into their composition, and therefore it is not, as has been supposed, a manifestation of the existence of any endowment which may be possessed by the "red corpuscles" by virtue of the iron which they contain. But even if this had been so, the fact above mentioned, of the presence of iron in the uncoloured or but little coloured corpuscles of the blood of the Crab, renders it extremely probable that what endowment soever may be possessed by the "red corpuscles" of the Vertebrata by virtue of the iron which they contain, may be equally possessed by the corpuscles of the blood of the Crab.

Professor Wagner adduces arguments of another kind in corroboration of his view, that the circulating fluid of the Invertebrata is "mere chyle, not proper blood;" but which it is scarcely necessary to notice after what I have just said, especially as their force, so far as it is evident, appears to bear more against than in favour of his view.

VII. *On a point connected with the dispute between* KEIL *and* LEIBNITZ *about the invention of Fluxions. By* A. DE MORGAN, *Esq., F.R.A.S., &c. Communicated by* S. HUNTER CHRISTIE, *Esq., Sec. R.S., &c.*

Received November 27, 1845,—Read January 29, 1846.

THOSE who have consulted the records of the Society for purposes of history, have omitted to find, or at least to notice, an addition made to the Committee appointed to decide between KEIL and LEIBNITZ, between the commencement of its labours and the presentation of its report. Singularly enough this omission makes NEWTON appear to assert a positive falsehood, in a matter of which he must have had accurate knowledge, and as to which the position of President gave him every opportunity of refreshing his memory.

The real facts are, that on the 6th of March 1711–12, a Committee was appointed to inspect the Archives, consisting of ARBUTHNOT, HILL, HALLEY, JONES, MACHIN, and BURNET: and that further, ROBARTS, a contributor to the Transactions, was added on the 20th, BONET, the Prussian minister, on the 27th, and DE MOIVRE, ASTON, and BROOK TAYLOR on the 17th of April. Of these newly-added members, BONET and DE MOIVRE were aliens. All the historical writers who enumerate the Members of the Committee, say nothing about the additions, but confine their list to the six gentlemen first named, that is, to the original Committee.

NOW NEWTON, in his letter to CONTI of February 26, 1715–16, first published at the end of the English edition of RAPHSON's History of Fluxions, asserts that the materials of the *Commercium Epistolicum* were "collected and published by a *numerous* Committee of gentlemen *of different nations*, appointed by the Royal Society for that purpose."

Whether a Committee of six is properly called numerous may be a question; but there can be no doubt that knowingly to have styled ARBUTHNOT, HILL, HALLEY, JONES, MACHIN, and BURNET, a committee of gentlemen of different nations, would have been, if not a direct falsehood, at least the pitiful evasion of treating the four races of the United Kingdom as four different nations with reference to a dispute between an Englishman and a German. As to this, it is material to remember that the names of the Committee had never been published, nor does NEWTON give them in the place cited. If any one should say that MACHIN's name might have looked rather French to an Englishman, it might be answered that NEWTON must have known JOHN MACHIN of Gresham College, whom he was constantly in the habit of seeing, to have been as much a Briton as himself. And in fact, MACHIN looks so little like a French

name, that LAPLACE once spells it MECHAIN, meaning BRADLEY's friend, not his own contemporary. In this state, owing to the accidental omission of the additions made to the Committee, the evidence of NEWTON's veracity was left: and considering the number of insinuations made by the partizans of the English side of the dispute against the moral character of LEIBNITZ, there can be little doubt that continental writers on the other side would have hit this apparent blot, if they had been able to see it. An accidental passage in a memoir of DE MOIVRE led me to suspect the omission above described: and the Assistant Secretary, on my application, was good enough to examine the records of the time and to supply it. The biography to which I allude is the "Mémoire sur la vie et sur les écrits de Mr. ABRAHAM DE MOIVRE .... par Mr. MATY. A la Haye, de l'imprimerie de H. SCHEURLEER, F. Z." (12mo, no date). This Mr. MATY must have been Dr. MATTHEW MATY, who, as well as his son, was a secretary of the Society. He was the intimate friend of DE MOIVRE, and he mentions Dr. BIRCH as having consulted the registers of the Society for him on different points. He states that DE MOIVRE was appointed on this committee on the 17th of April (the true date), and gives *all* the names. He also states that this transaction drew DE MOIVRE out of the neutrality which till then he had observed. It is remarkable that MATY thus intimates that the mere fact of joining the Committee was destruction to the character of a neutral. And BURNET himself, a member of the Committee, is stated by JOHN BERNOULLI (in his correspondence with LEIBNITZ) to have written a letter to him, giving him the information that the Royal Society was engaged in proving that LEIBNITZ might have seen certain letters of NEWTON, &c. These slight things tend to show that the Committee in question was thought at the time not to be a judicial body, but one of avowed partizans: and it is, I think, necessary for the character both of the Committee and the Society, that this truth, as I believe it to be, should be acknowledged. On any other supposition than that the Committee was meant to be Newtonian, for the defence (through KEIL) of NEWTON, and not for the decision of the question, it would be difficult to explain with credit to the parties concerned, the selection of ARBUTHNOT, BURNET, and ASTON, friends of NEWTON, but not known as mathematicians; or of BROOK TAYLOR, a Fellow of a few days standing, who had then published nothing, and was only known to the Society, or chiefly known, by his private correspondence with KEIL, the nominal defendant. There may have been, and I often suspect there was, something of truth in the surmise of LEIBNITZ, who thought that the near prospect of the Hanoverian succession created some dislike against the subject and servant of the obnoxious Elector in the minds of the Jacobite portion of English science. "Amicus Anglus ad me scribit," says LEIBNITZ, "videri [eos qui parum Domui Hanoveranæ favent] aliquibus non tam ut mathematicos et Societatis Regiæ Socios in Socium, sed ut Toryos in Whigium quosdam egisse." I am unfortunate enough to differ from the general opinion in England as to the manner in which LEIBNITZ was treated, and this after much examination: I am therefore the better qualified to call the attention

of the Society to the groundlessness of the charge which might, from an accidental omission, have been made against NEWTON's veracity; and I am glad it has fallen to me to do so.

I can hardly find any notice of this little tract of Dr. MATY. Had it been much known I cannot but suppose that the following phrase of NEWTON would have obtained some currency: the English parenthesis, and the italics, are MATY's, not mine. "Comme tout ce qui regarde les grands hommes peut être intéressant, on sera peut-être bien aise de savoir que NEWTON a souvent dit à Mr. de MOIVRE que s'il avoit été moins vieux il auroit été tenté de revoir sur les dernières observations sa théorie de la Lune, ou comme il s'exprimoit *de l'attaquer de nouveau* (*to have another pull at the moon*). Je tiens ceci de Mr. de MOIVRE lui-même."

There is also an interesting fact concerning DE MOIVRE, which is little, if at all, cited. Happening one day to have to call on the Earl of Devonshire, he met a man whom he did not know coming out of the house. This man was NEWTON, who had come to present a copy of the *Principia*, and had left it in the antechamber in which DE MOIVRE had to wait till the Earl was ready to see him. He opened the book, and found to his surprise, that, strong as he knew himself to be, he could only just manage to follow the reasoning. He immediately procured a copy; but as he was employed all day in walking about London from one pupil to another, the only way in which he could read it was by tearing leaf after leaf, and carrying a few at a time in his pocket to look at in his walks and in other intervals.

It cannot be necessary to remind the Society that the subject of this paper may possibly not be the only case in which their records would furnish information on points of history which have hitherto been overlooked. These records are a most important part of the progress of science in England: and I have often regretted that the example set by Dr. BIRCH has found no follower among those who can appreciate the value of minute information.

## VIII. *On the Action of the Rays of the Spectrum on Vegetable Juices. Extract of a Letter from Mrs.* M. Somerville *to Sir* J. F. W. Herschel, *Bart., dated Rome, September* 20, 1845. *Communicated by Sir* J. Herschel.

Received November 6,—Read November 27, 1845.

In the following experiments the solar spectrum was condensed by a lens of flint glass of $7\frac{1}{2}$ inches focus, maintained in the same part of the screen by keeping a pin-hole, or the mark of a pencil constantly at the corner of the red rays, which were sharply defined by using blue spectacles to protect my eyes from the glare of light, and the apparatus was covered with black cloth in order to exclude extraneous light.

Thick white letter paper, moistened with the liquid to be examined, was exposed wet to the spectrum, as the action of the coloured light was more immediate and more intense than when the surface was dry. As I had not access to the morning sun, the observations were made between noon and three in the afternoon.

The lavender rays came vividly into view under a condensed spectrum on white paper washed with a solution of sulphate of quinine in dilute sulphuric acid: they were narrow, and their length, by rough measurement, was equal to the distance between the upper edge of the violet and the lower edge of the blue. They were very brilliant on black silk or other dark surfaces, and invariably of lavender colour; and even on paper stained with turmeric, the pale yellow rays which you had observed, were tipped with lavender on being washed with this liquid, though its duration was momentary, as it vanished as the surface became dry; but they were permanent in other instances.

The lavender rays change their colour with a change of the liquid, for instance they are lavender coloured on nitrate of silver discoloured by light to a very pale brown, washed with a solution of sulphate of quinine in dilute sulphuric acid; whereas, on a similar surface of pale brown nitrate of silver, washed with the juice of the petals of pale blue *Plumbago auriculata* in distilled water, to which sulphuric acid was added, they appeared of a vivid apple-green, and acquired a tip of lavender colour on the surface being washed with a solution of sulphate of quinine in dilute sulphuric acid of considerable strength; the effect, however, was transient. After several unsuccessful attempts to repeat this experiment next day, I at length discovered that its success depended upon the acid being strong enough to decompose the juice and give it a reddish orange hue; and even then the rays are not vivid till the paper has been frequently washed with the juice and become nearly dry; and

the experiment is more successful when the liquid has been kept a night. The action of the surface in changing the colour of the lavender rays may be illustrated by passing the spectrum over paper coated with nitrate of silver brought to a clear yel low-brown by exposure to the sun; one half being washed with the liquid in question, and the other half with a solution of sulphate of quinine in dilute sulphuric acid, on the first half the lavender rays became vivid apple-green, while on passing to the other half they instantly change to an equally vivid lavender colour. These rays often darken the surface throughout their whole length; sometimes they acquire a powerful bleaching action, and sometimes they have no effect, as evidently appears from the following experiments.

There was reason to suspect that the action of the spectrum at the junction of the lavender with the violet rays, is in some cases different from what it is on either of these colours separately, a second dark image having appeared at the extremity of that which terminated with the violet, indicating a break in the continuity of action and giving the idea of a secondary spectrum.

The juice of fresh-gathered petals of double flowering pomegranate in alcohol afforded an example of this. Paper washed with this juice became rich crimson, and on being exposed wet to the condensed spectrum, a narrow line of deep crimson was formed at the junction of the green and yellow rays, or perhaps in the most refrangible yellow, surrounded by a whitish lozenge-shaped border (Plate III. fig. 1.). On again washing with the juice, instead of the white border, which had vanished, there was a crimson flame-shaped image, curved at the lower edge of the yellow rays and tapering upwards to the violet. Its colour was darker than that of the ground, though paler than the narrow line, which maintained its intensity; and although the latter increased in width, it did not become as broad as the image in question. At the upper end of the violet another little dark image was formed, apparently owing to the action of the lavender rays, having exactly their form. The orange and red rays, especially the red, had no effect, though at the distance of about half the length of the spectrum beyond the red, two distinct spots were formed of deep crimson, which I believe to be the heat spots which you discovered. After some time, a bleaching appearance surrounded the whole image from the red upwards, probably owing to rapid evaporation from the heat of the spectrum (fig. 2.). Exterior bleaching frequently took place in the course of the experiments, permanent in some instances, while in others it vanished as the surface dried. When water was used with the juice instead of alcohol, the general character of the image was similar to that described, except that the small figure beyond the violet was more distinct, and seemed to bear the same proportion to that formed by the rest of the spectrum which the length of the lavender rays bears to the length of the sum of the others. The bleached part round the whole was more extended, and a faint crimson haze encompassed the dark spots, which were very distinct (fig. 3.).

This experiment is one of many instances in which I have observed the powerful

influence of the yellow and green rays upon vegetable substances, an influence which seems to be unconnected with heat, since the darkening generally was least under the red rays, and immediately below them, where the calorific rays are most abundant. I found that the action of the rays of mean refrangibility on vegetable juices was much increased by the addition of a little sulphuric acid. In most cases this powerful influence was simultaneous with the appearance of two dark spots below the visible spectrum, sometimes sharply defined, sometimes in the centre of a disc of a paler hue; and in some instances the disc was seen without spots, though it rarely happened that they did not appear under one or other of these forms.

The following are some of the cases in which the simultaneous effect was produced: for example, paper washed with juice of the petals of *Globe amaranthus* in distilled water, on exposure to the spectrum acquired a delicate pink tint, which was soon bleached to whiteness, from the upper edge of the green to the end of the lavender rays, while at the same time a perfectly circular spot of equal whiteness was seen under the red rays, and a little way below them, which had the appearance of being an image of the sun. After more washing with the juice, the two bleached parts were united by a long white neck, which speedily vanished, and was succeeded by a dark crimson image, whose greatest intensity of colour was under the yellow rays. At some distance below the red rays, two crimson spots were strongly marked, especially the uppermost, both surrounded by a paler halo (fig. 4.).

The juice of the petals of pale blue *Plumbago auriculata* in distilled water imparted its tint to writing-paper, which, after exposure to the action of diffused light, acquired a pale yellowish green hue. The part under the lavender and violet rays of the spectrum, repeatedly washed with the juice, assumed a pale brown colour: the indigo rays seemed to have no effect, although from their lowest edge to the distance of half the length of the spectrum below the red rays, a lavender blue image was formed. Under the orange rays a minute indigo-coloured spot appeared, and also a larger spot of the same colour under the yellow, which were soon blended into one, forming a single oblong figure of maximum intensity, surrounded by a halo of paler indigo (fig. 5.). An insulated disc of the same colour as the halo, with two dark spots in its centre, appeared at some distance below the red rays. The juice of this plant is easily decomposed; by the addition of sulphuric acid it became a delicate pinkish grey, which was also the colour of paper wetted with it, while under the spectrum a pale yellow figure of considerable width was impressed, extending under the whole of the visible rays, and soon after a deep yellow image with a paler halo was seen in its centre. The halo or paler part of the image lay between the red and the end of the lavender rays, while the deep yellow, or perhaps more properly the yellow-brown figure, extended under the yellow-green and blue rays, the maximum of breadth and of colour lying in the yellow and green. Two insulated spots of the same yellowish-brown in the centre of a yellow disc were formed below the visible spectrum, and the whole surface soon became bright yellow from the action of the scattered light, conse-

quently the extensive pale figure was obliterated, leaving only the interior dark image with its halo and the spots with their disc permanent.

The juice of beet-root in a strong solution of common salt imparted a pink colour to the paper, and the most refrangible rays acquired a powerful bleaching energy. The pink ground was whitened under the lavender, indigo and blue; a deep crimson spot was formed under the yellow, with a rose-coloured halo elongated to the bleached part on one side, and to the end of the orange on the other, while a hazy rose-coloured disc was visible at a distance below the red (fig. 6.). The crystallization of the salt on this figure was in proportion to the intensity of colour; most on the crimson spot and its halo and on the coloured disc, but scarcely any on the bleached portion.

A solution of hyposulphate* of potash in the beet-juice had nearly the same effect as the salt, only that the dark spot with its halo was more extended, and the latter formed a long neck beyond the red, ending in a deep rose-coloured disc. The appearance was precisely similar when muriate of ammonia or iodide of potassium was used, I therefore inferred that the alkali imparted the bleaching property to the most refrangible rays.

Paper washed with the juice of beet-root in distilled water assumed a fine rose-colour, on which a figure was bleached by the spectrum, wide and rounded in the yellow, but tapering upwards: after another wash, a dark crimson image was formed in its centre reaching to the blue, with a still darker spot under the yellow. The red rays had no effect, below which at the usual distance two indistinct spots appeared. A drop of sulphuric acid in the juice changed the tint of the paper to a purple colour, on which, after various alternations of bleaching and darkening, the spectrum impressed a dark figure, broad and rounded on the yellow, and tapering to the blue, beyond which there was scarcely any action (fig. 8.). The whole was surrounded by a broad pale border, stretching in a neck far beyond the red, with an indefinite dark line in its centre.

In these experiments almost the only part of the visible spectrum that darkened the surface was that of mean refrangibility, but even when the whole had that effect, the maximum was generally produced by the rays in question, the insulated spots, or some indication of them, appearing at the same time. This was the case with the juice of *Amaranthus caudata*, which stained white paper rose-colour, and the spectrum impressed a deeper rose-coloured image from the lowest edge of the red to the end of the blue, with a dark crimson oval figure under the yellow and green, while two insulated crimson spots appeared in a rose-coloured halo below.

White paper washed with the juice of rose-coloured verbena in distilled water assumed a lemon-colour (fig. 9.), while the spectrum impressed a pale rose-coloured image from the lowest edge of the red to the end of the indigo rays, in the centre of

* *Sic in MS.* but probably meaning hyposulphite, a salt in common use in photographic research, which the hyposulphate is not. The two salts have totally different properties. The mistake is a very common one.—(J. F. W. H.)

which, under the green, yellow, and part of the orange rays, a narrow figure of very deep crimson was formed, and two rose-coloured spots appeared in a rose-coloured disc below the red.

Dark orange-coloured nasturtium stained writing-paper the colour of prussian blue, the image impressed on which was a deeper shade of the same, extending from the lowest edge of the red to the end of the blue, while a still deeper tint appeared under the orange, yellow and green, with a point of maximum intensity in the lowest part of the yellow. There was a tendency to bleaching from the blue upwards, and below the other end of the spectrum two well-defined dark spots were surrounded by a paler blue penumbra (fig. 10.).

The juice of the velvety petals of a scarlet geranium was very sensible to the action of light (fig. 11.), and stained white paper rose-colour. A deeper rose-coloured tint was impressed by the spectrum from the lowest edge of the red to the end of the blue, feeble in the red and deeper in the orange, while a crimson image extended from the zero point in the yellow to the end of the green; the maximum of intensity was in the latter. Two crimson-coloured spots in a rose-coloured halo were strongly marked at some distance below the red.

With the addition of acid this juice gave a scarlet tint to paper, and a darker scarlet image was formed from the middle of the yellow to the end of the indigo. Then a very dark scarlet oval appeared between the zero point and the upper edge of the blue, with a small point of maximum intensity on the upper part of the yellow. The orange and red rays had no effect, but a pale rose-coloured border surrounded the lower part of the figure, which became of a bluer tint and darker than the ground when dry. Two spots of scarlet were distinct at some distance below (fig. 12.).

The juice of the petals of scarlet balsam in distilled water is another instance; it gave a pale scarlet tint to paper, and a lake-coloured image extended throughout the spectrum from a point considerably above the violet to below the spots, which were distinctly visible, though not sharp. The greatest intensity of colour and breadth were under the yellow and green. When a little sulphuric acid was added to the same liquid, the action was feeble above the blue rays, but a very broad dark image was formed under the orange, yellow and green rays; possibly under part of the blue also, which gradually became nearly black, with a blackish red penumbra, surrounded by a broad border of bright scarlet of a deeper tint than the ground, passing under the blue and indigo at one end, and under the red at the other, while below the latter an indistinct appendage of a paler colour projected; an insulated scarlet disc appeared still lower down, in which two spots were evident, though not sharply defined (fig. 13.). The breadth of the image was very remarkable, and it was so deeply impressed that there was little difference in its intensity on the two sides of the paper. On repeating the experiment with a hot iron behind the paper, the figure was the same, only that the central part was brown and the border merely a paler shade of the same: a

disc of the same colour as the border appeared below the red, which vanished the following day.

When the juice was extracted in alcohol instead of water, the action of the spectrum was somewhat different; the image extended from the lowest edge of the yellow to the end of the violet with three points of maximum intensity, the greatest in the yellow, the next in the green, and the last in the indigo. The red rays had no effect, but the spots, or rather their place was indicated. With water containing a little borax, there were only two points of greatest intensity, the first on the yellow, the other under the indigo and violet. Notwithstanding these differences the principal action was still in the rays of mean refrangibility, especially the yellow.

I have found no vegetable juice so sensible to light as that of the maroon-coloured or dark red dahlia in distilled water. It gave a lavender hue to white paper, while the part under a condensed spectrum became rapidly dark, from the edge of the orange to the end of the lavender rays, but with variable colour and intensity. A dark reddish oval was formed under the orange, yellow and green rays, with a narrow line of a still deeper colour in the centre of the yellow, while spots of less intensity and of a bluer tint appeared under the blue and violet rays, with some indication of another above; but these three points of inferior intensity were not always very distinct. At a little distance below the extreme red rays, two dark spots were formed in the centre of a hazy disc of reddish purple. A hot iron behind the paper occasioned no change in the figure, though it accelerated the action and increased its sharpness. In this and various other instances I found that repeated washing with diluted juice answered better than when it had a deeper colour (fig. 14.).

The addition of a little sulphuric acid to this juice accelerated the action of the rays of mean refrangibility, increased the intensity and breadth of the image, and changed the tint of the liquid to bright scarlet, which imparted its hue to white paper. Exposed wet to the spectrum a dark red figure by degrees appeared, extending from the upper edge of the orange to the end of the violet, in the midst of which a kind of oval of great intensity of colour was formed under the yellow and green rays, leaving a broad red border around. The red rays seemed to have little or no effect, though at some distance below them there were two dark spots on a dark haze. When the experiment was repeated with a hot iron behind the paper, the central image became almost black, and extended from the lowest edge of the yellow to almost the middle of the blue rays, while the dark crimson border passed under the orange at one end of the spectrum, ending at the indigo at the other, beyond which a pale crimson shade reached certainly to the end of the violet, and probably further, but it was so gradually shaded off that it was not possible to define its termination. There was a nebulous disc of pale crimson at some distance below the last visible red. This image, as well as that in the preceding experiment, were so deeply impressed that they penetrated the paper (fig. 15.).

Various other plants might be mentioned to show the peculiarly strong influence of the rays of mean refrangibility in darkening vegetable juices, and in this respect there seems to be a difference in the action of the spectrum on vegetable matter and metallic salts; it would however require a more extensive series of experiments to establish this point.

The very frequent recurrence of two insulated dark spots, or of some indication of them, is another peculiarity in the action of the spectrum on by far the greater number of vegetable juices which I have used. If these be identical with the heat spots discovered by you, there surely must be a difference in the nature of the calorific rays in the visible and invisible part of the spectrum, since these spots, or a darkish disc, often appeared when the red rays had little or no effect, or even possessed a powerful bleaching energy, as on the juice of the violet-coloured *Globe amaranthus*.

Since the simultaneous appearance of the insulated spots and the maximum intensity of darkening under the rays of mean refrangibility occurred so often, I am led to suspect a similarity, possibly identity, in the nature of the agent producing these phenomena. The greatest heat does not lie in the mean rays of the spectrum, nor does the greatest chemical action; and as you have shown that the parathermic rays are situate there, may not they be the cause of the phenomena in question, being also mixed with those rays of caloric which form the heat spots, and which I have no doubt are identical with the insulated spots in these experiments?

Some cases have occurred, and there may be many more, in which the maximum intensity was produced by those rays in which the chemical energy is most active, as for instance in the juice of *Coreopsis tinctoria*, which was darkened from the red to the end of the violet rays with almost equal intensity; the maximum, if there was any, lay under the blue and indigo; also the juice of the red and yellow variegated *Marvel of Peru*, which was darkened under the whole of the visible spectrum, the lavender rays excepted. But by far the greater number of vegetable juices which I have examined, were most affected by the most luminous rays.

The point of maximum intensity was sometimes altered by the addition of an acid or alkali, and sometimes by using alcohol instead of water; when a drop of acid was added to the juice of the crimson marvel of Peru, the greatest intensity of colour appeared under the indigo and blue, and a little carbonate of soda changed the point of greatest intensity from the yellow and green to the blue rays in the juice of *Plumbago auriculata*. A still greater change was produced on the juice of the scarlet geranium by the addition of carbonate of soda. The colour of the liquid was violet, but it stained the paper pale bluish-green. There was no action under the red rays, but a circular spot of a decided yellow colour was formed under the orange and yellow rays. A brownish tint was impressed by the green and blue, from the end of which up to the termination of the lavender rays, the ground assumed an inky colour of considerable intensity, with a strong maximum in the violet. At the usual distance below the red, two green spots appeared (fig. 16.).

The action of the different parts of the spectrum seems very capricious; for example, the most refrangible rays from the green to the end of the lavender darken some substances and bleach others. Instances of this bleaching action have been given in the juices of *Globe amaranthus* and *Plumbago auriculata*, both being bleached to whiteness by the rays in question. The red rays occasionally possess this property, as in the juice of the *Globe amaranthus*, those of mean refrangibility more rarely, yet an instance has already been given in the case of geranium juice with carbonate of soda; and another occurred when a drop of nitric acid, a little common salt, and sulphate of quinine were mixed with the juice of the dark red dahlia in distilled water. White paper repeatedly washed with this liquid acquired a tint of pale lilac, while the part under the spectrum was marked by a broad dark crimson image from the orange to the end of the violet. After another washing the paper was bleached to a very pale yellowish-white under the lavender and violet rays, while at a small interval below a second bleaching process began, and changed the interior of the dark image to a very pale reddish-yellow down to the beginning of the orange, leaving a dark crimson border on each side only, broadest and darkest under the yellow rays, and diminishing in width both up and down. Under the orange and red rays the surface acquired a pink tinge, and at some distance below, the same pink tinge formed a round hazy mark. Although the colours were very tender in this experiment they penetrated letter-paper, remaining unchanged many days (fig. 17.).

Singular changes were produced by the spectrum on the juice of the petals of the scarlet *Zinnia* in distilled water, which though not very sensible to the influence of light alone, became highly so on the addition of sulphuric acid. White paper washed with this liquid acquired a pinkish-grey tint, on which the first effect of the spectrum was a very broad dark image under the violet and indigo, with a long narrow appendage of the same inky tint going down to the end of the red, and also a dark shade upwards far beyond the violet. Being again washed with the liquid, the broad image under the violet and indigo was bleached white, leaving a dark margin (fig. 18.). The dark appendage then gradually became broader, the greatest intensity lying under the green and yellow, and a dark disc appeared below the red (fig. 19.). After another washing a bleaching action changed all the interior of the dark appendage, from the upper edge of the green to the end of the red rays, to a very pale buff colour: after a while a yellow brown or dark buff image appeared in its interior under the orange, yellow and green, leaving a very dark margin; and now the whole image was surrounded by an inky pink-coloured border, much wider than the visible spectrum, extending on a long neck to the dark disc already mentioned, in which the uppermost spot was seen, but the whole of the dark border had vanished next day.

I have not found much variety of colour, although it was occasionally remarkable: the juice of the thread-shaped petals of a yellow thistle in distilled water gave a bright yellow colour to white paper, on which the spectrum impressed a very long narrow image throughout the whole visible rays, varying in colour and intensity.

The darkest part lay between the lowest edge of the blue and the highest extremity of the lavender rays: that under the lavender was bright red, while the part under the violet, indigo and blue, was orange-coloured. From the blue to the end of the red was deep yellow, and below the red rays two insulated spots of the same deep yellow were formed, though somewhat blended together. On the juice of *Plumbago auriculata* the lavender and violet rays produced a pale brown image; the indigo rays had no effect, while all the rest of the image under the mean and least refrangible rays was blue and indigo; some of the juice having remained till next day, the moisture had evaporated from it, leaving a yellowish-brown sediment in the bottom of the cup, surrounded by a border of indigo, the very same colours that were separated by the solar rays and impressed upon paper the day before, showing a striking analogy between the action of the two ends of the spectrum and that of the poles of a galvanic battery. There was a green ring between the brown and the blue where the two colours were blended, and this also was visible in the greenish-yellow tint of the general ground of the paper after exposure to light. I now expected to find that paper washed with the brown sediment, dissolved in distilled water, would be impressed with the brown part of the image only, and this was the case, with the exception of some slight indication of brown at the place of the spots. There was not enough of the blue pure to enable me to obtain the indigo part of the figure alone.

The variation in the width of the images impressed by the same spectrum on different vegetable substances was remarkable; on the juice of the yellow thistle already mentioned, the image was but a mere line, nor was it much broader on the juice of Plumbago: it was wider on the juice of the dahlia, but the addition of a drop of sulphuric acid increased the width greatly, as in the images formed on the juice of beet-root, the petals of dahlia, balsam and others. Possibly the cause of this may have been the dispersion of the rays by the acid, which was particularly great in the most luminous rays of the spectrum, though in various instances, where acid was not used, the action of the spectrum seemed to extend laterally beyond its visible edges. This occurred especially when a surface of slightly browned nitrate of silver was washed with vegetable juices or other liquids. Under these circumstances the image frequently consisted of a succession of borders of various breadth and intensity surrounding a central oblong nucleus, resembling the section of a bulbous root cut longitudinally and exhibiting its coatings. For example, a slightly sunned surface of nitrate of silver, washed with the juice of the young green walnut in a strong solution of common salt became lilac, though the liquid itself was brownish; but under the spectrum an olive-brown image was formed from the upper edge of the orange rays to a considerable distance beyond the violet, where it tapered to nearly a point. After two or three washings with the liquid, the internal part was changed to a paler tint from the indigo rays to below the red. The part projecting down from the red was much narrower than the rest of the figure and ended in a curve. Then a dark and almost elliptical figure was formed within the pale part inclosing part of the

paler surface, and in the centre of which a very dark oblong spot appeared; thus from the exterior lilac ground on each side of the image to the central spot, there were five changes of intensity and tint, reckoning the central spot, but not the lilac ground; and though several of these were mere lines, the whole figure was considerably larger than the visible spectrum (fig. 21.). Another remarkable instance occurred, both of these bands and of the breadth of the image, when paper coated with white nitrate of silver was washed with the milky juice of the fig-tree in alcohol, exposed wet to the spectrum. The first effect was a fine clear brown image, beginning with a curve at the orange, and extending far beyond the violet rays, within which an olive-green centre began to appear under the green rays, which by degrees extended to the end of the violet. The brown edges bordering the olive-green were strongly marked, and seemed to spread beyond the sides of the visible spectrum throughout the violet, indigo, blue and green, but were united by a very black round spot under the yellow, and this spot was inclosed on the under side by a brick-red cusp stretching up on each side beyond the orange rays, at the lower end of which was a circular spot of very pale yellowish-white; the whole figure was surrounded by a broad reddish border, and after long exposure the centre of the green assumed a dark greyish hue, so that there were various tints, estimating laterally from the ground to the centre of the figure (fig. 22.).

Juice of the dark purple flower of a kind of mint in alcohol afforded another instance of several bands. The liquid gave the paper a brownish tint, on which the spectrum impressed a long dark line about the breadth of a common pin, cut off by the orange rays, but reaching to the end of the violet. On being again washed with the juice, the dark brown line gradually increased in width, taking a curved termination in the yellow, where it was broadest and tapering upwards; the interior afterwards assumed a pale hue, leaving a dark margin; the whole at last was surrounded by a pale border bounded by a very dark line, which were barely visible next day. Two insulated dark spots were distinctly marked below the image.

These out of many instances are sufficient to show the peculiar appearance alluded to, which it is difficult to account for, unless that at each successive washing the heat of the spectrum, by drying the parts immediately around it more rapidly than the rest of the surface, may thus produce the various borders, and extend the figure beyond its visible edges; however, some of the other phenomena could not be so explained, and possibly may have been connected with the action of the atmospheres of the sun or earth, as you have remarked, or to a difference in the action of the rays at the edges of the sun's disc from those in its centre. I fear I may have made some mistakes, especially in the estimation of the action of the different coloured rays, the limits of which it was extremely difficult to determine in so small a spectrum as that with which I worked.

## IX. *On the Use of the Barometric Thermometer for the Determination of Relative Heights.* *By* James R. Christie, *Esq., of the Royal Military Academy. Communicated by* S. Hunter Christie, *Esq., Sec. R.S., &c.*

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ALTHOUGH the observation of the temperature of boiling water has been for some time, but not extensively, employed for the determination of relative heights, yet the only means which experiment has confirmed of reducing it to a measure of the atmospheric pressure as usually estimated by the height of an equiponderate column of mercury has, till very recently, been overlooked; and it may perhaps be owing to this circumstance that the instrument for making the requisite observations remains to have fully developed in it the advantages it undoubtedly possesses, in portability and strength of construction, over the fragile and easily deranged barometer.

My attention having been called to this subject by a remark made by Professor Forbes in his interesting work on the Alps, to the effect that he had found the temperature of boiling water to decrease uniformly with the increase in height of the place of observation, and at the rate of one degree of Fahrenheit for every 550 feet of vertical ascent, I considered that it would be highly satisfactory to verify this result during an excursion over the Alps of Savoy and Piedmont which I then had in contemplation, and in the course of which I proposed to visit some localities at very considerable elevations above the sea level: and I was induced also to seek for some foundation for this very simple law. In prosecuting the latter inquiry, I soon found that, by assuming the truth of De Luc's formula for the determination of the boiling-point from the barometric pressure, at all accessible heights, a corroboration of the law in question is at once arrived at. I have since found, by reference to a paper in Vol. xv. of the Transactions of the Royal Society of Edinburgh, that Professor Forbes had himself verified his original conjecture in the same manner.

The formula alluded to, when reduced to the common English units of measure, becomes

$$b = \frac{99}{\cdot 899} \cdot \log 10\beta - 60\cdot 804 \quad \ldots\ldots\ldots\ldots \quad \text{I.}$$

where $b$ is the variable boiling-point on Fahrenheit's scale, and $\beta$ the corresponding barometric pressure in inches of mercury. From this we obtain at once

$$\log 10\beta = \frac{\cdot 899}{99}(b + 60\cdot 804)$$

and, calling $\beta'$ and $b'$ two other corresponding values of $\beta$ and $b$, we have

$$\log\beta - \log\beta' = \frac{\cdot 899}{99}(b-b').$$

Substituting this value in Laplace's formula for the determination of H, the number of English feet in vertical height between two stations, where the barometric pressures are $\beta$ and $\beta'$, and the mean temperature of the air at which is $t$, viz.

$$H = 60345\cdot 6(\log\beta - \log\beta')\{1+(t-32^\circ)\cdot 00222\},$$

we finally get

$$H = 547\cdot 99(b-b')\{1+(t-32^\circ)\cdot 00222\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{II.}$$

That a very high degree of accuracy attaches to the formula I. is at once evident from the observations made, in test of it, by its author, and more especially from the one made by De Saussure on the summit of Mont Blanc with the same object. I proposed therefore, in undertaking a series of observations at elevated places, to restrict myself to the question of the fitness of the barometric thermometer, in its present form, for affording trustworthy data; making observations at stations whose heights had been previously well-determined, and at as great elevations as circumstances would admit: and, in case of feeling satisfied on this point, I considered that it would be interesting to determine the extent of accuracy to which such a series of observations would give the relative levels of successive places of observation, without reference to the simultaneous observations at any fixed station.

The instrument with which my observations were made consists of a brass cylindrical boiler, 3 inches in depth and 2·25 inches in internal diameter, supported, when in use, upon a brass tripod stand; in the upper disc of this cylinder a thermometer, graduated from 181° to 215° Fahr., is made to screw by means of a strong brass collar, one inch in diameter, surrounding the tube at the distance of two inches from the bulb, which is of a pear-shaped form, having the diameter of its largest circular section ·75 inch. The whole length of the scale is rather more than twelve inches, and each degree is ·343 inch in length, subdivided into tenths, and reading, by means of a vernier, to hundredths; the index at the zero point of the vernier by enveloping the tube prevents the intrusion of parallax. The water in the boiler is heated by means of a small spirit-lamp, which I have always found sufficient for that purpose, even in situations exposed to considerable force of wind, when protected by a shade formed by coiling up and pinning together the ends of a sheet of stiff paper. The escape of steam is at a small orifice, ·15 inch in diameter, in the upper disc of the boiler, and also through the aperture left between the tube of the thermometer and its screw collar. In using the instrument I was careful to have the water as nearly as possible of the same depth on all occasions, and to make use of the purest I could procure, which was apparently always sufficiently free from any admixture of foreign matter likely to cause appreciable error in the boiling-point.

It was necessary, before deducing any results from observations made, to deter-

mine whether the instrumental graduations could be relied on; and not having had an opportunity of comparing the instrument with any standard barometer in England, I was under the necessity of employing my Geneva observations for that purpose, comparing them with the corresponding indications of the barometer at the Geneva observatory, the height of which I estimated at seventy feet above the spot at which my observations were made. This difference of level will amount to

$$\frac{70}{548} = \cdot 128^\circ \text{ Fahr.}$$

to be added to the boiling-point as deduced from the height of the barometer. These corresponding observations were

*Geneva.*

| Observed boiling-point. | Reduced barometer. | Calculated boiling-point. |
|---|---|---|
| 208°·820 | ·72129 metre. | 209°·357 |
| 209·115 | ·72345 metre. | 209·500 |

We have then $209 \cdot 357 + \cdot 128 - 208 \cdot 820 = \cdot 665$ error,

and $209 \cdot 500 + \cdot 128 - 209 \cdot 115 = \cdot 513$ error

$2)1 \cdot 178$

Mean error . . . . $= -\cdot 589$

The non-accordance of the errors I am inclined to attribute to the latter pair of observations not being simultaneous, mine being made at 6 A.M. and the barometric indication being deduced from that recorded at 9 A.M., which is the earliest.

It was at once evident to me that the above error was not likely to afford a constant correction to be applied to all observations made at different points of the scale, because the lower point of graduation, being necessarily above the freezing-point, must have been determined by reference to a standard thermometer, and it was therefore not at all probable that it would differ from the truth by so large a quantity. An excellent opportunity of proving the truth of this conjecture offered itself at the Convent of the Great St. Bernard, where a register is kept of the variations of the barometric column. Arranging these as before, I obtain the results:—

*Great St. Bernard.*

| Observed boiling-point. | Reduced barometer. | Calculated boiling-point. | Error. |
|---|---|---|---|
| 197°·640 | ·56538 metre. | 197°·709 | −·069 |
| 197·680 | ·56630 metre. | 197·787 | −·107 |

There was a difference of level of ten or twelve feet between the two places of observation, the first having been made in the salle-à-manger of the Convent, and the other in one of the dormitories on the next floor where the barometer is fixed. The correction for this change of level is

$$-\frac{12}{548} = -\cdot 022$$

to be added to the error of the first observation, which becomes then $-\cdot091$; and the mean error derived from the two observations is $-\cdot099$.

The instrumental error thus diminishing appears to indicate that there is a point on the scale where it becomes 0, and beyond which, as a correction, it must be applied in a negative form; and this point can readily be determined on the principle that the differences of the errors are proportional to the differences of the corresponding boiling-points; for, calling B the point on the scale at which the error vanishes, $b$, $b'$, $e$ and $e'$ two other points and their corresponding errors, we have the equation

$$\frac{b-B}{b-b'}=\frac{e\pm0}{e-e'};$$

whence

$$B=b-\frac{e.(b-b')}{e-e'};$$

and, substituting in this the values above found, viz.

$$b=208^{\circ}\cdot967,\ b'=197^{\circ}\cdot649,$$
$$e=-\ \cdot589,\ e'=-\ \cdot099,$$

we find $B=195^{\circ}\cdot363$.

In order, by means of this result, to determine the correction to be applied as a multiplier to the number of degrees above or below B, it remains to determine the amount of error due to each degree, which, in fact, when its sign is changed, will be the required factor; calling this correction C, we have

$$C=\frac{-e}{b-B}=\frac{\cdot589}{13\cdot604}=\cdot0433.$$

It may be remarked, that the principle on which the determination of this correction is founded remains unaffected by the supposition that the errors from which it is derived are partly attributable to the diameter of the bore of the thermometer increasing uniformly in descending; this I have satisfied myself by the usual means to be the case, though to a comparatively trifling amount.

The observations recorded at the Geneva Observatory and those at the Great St. Bernard afford an excellent means of applying my own to the determination of the height of my several stations, since those places present the advantage of being, the one my starting-point, and the other a position central to my series of stations. The readiest way of thus applying them appears to be, to reduce the height of the barometric column as given in millimètres to the form of the corresponding temperature of boiling water, by means of De Luc's formula above given, and then, for the determination of the difference of altitude between each station and Geneva, and the Great St. Bernard, to employ the formula

$$H=\{548(b-b')\pm24D\}\{1+(t-32)\cdot00222\},$$

where D is the difference in degrees between B($=195^{\circ}\cdot4$) and the observed boiling-point; and the factor 24 is the product $548\times\cdot0433$; the sign + being used when my own station is *lowest* and *vice versâ*, because the instrumental error is always one of defect.

Thus, taking the observations made on the Bonhomme and the corresponding ones at Geneva and the Great St. Bernard, we have,

Barom. Geneva. Boiling-point.
720·97 mm. gives 209·335

Obs$^{d}$ boil$^{g}$-p$^{t}$ 197°·700 197·700

195·4 11·635 × 548 = 6376

2·3 × 24 = 55 55

6321 × 1·059 = 6694 above Geneva Obs$^{y}$.

1334 Observ$^{y}$ above sea.

Croix du Bonhomme above the sea 8028 feet.

Barom. G.S.B. Boiling-point.
563·94 mm. gives 197·587

197·700

·113 × 548 = 61·9

55

116·9 × 1·0357 = 121 below Great St. Bernard.

8170 Gt. St. Bernard above the sea.

Croix du Bonhomme above the sea 8049 feet.

The following Table shows the simultaneous observations at the two fixed stations and at the variable one, and the results derived from them by the above method.

TABLE I.

| Station. | Observed boiling-point. | Temperature of the air, FAHRENHEIT. | Geneva barometer (reduced to 32° F.) in millimetres. | Geneva external thermometer. | Great St. Bernard barometer (reduced to 32° F.) in millimetres. | Great St. Bernard external thermometer centigrade. | Calculated height in feet above Geneva observatory. | Calculated depression in feet below the Great St. Bernard. | Sum of the two preceding (dash true). | Height in feet above the sea by Geneva observation. | Height in feet above the sea by G.S.B. observ. | Height in feet as previously determined. | Authority for the preceding column. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Les Rousses | 204·48 | 57 | 722·01 | 19·4 | 566·66 | 3·9 | 2644 | 4024 | 6668 | 3978 | 4146 | 3919 | WOEIL. |
| Geneva | 208·82 | 71 | 721·29 | 19·5 | 563·98 | 8·2 | (−70) | 6862 | 6792 | ..... | 1308 | 1334 | Bib. Univ. de Génève. |
| Bonneville | 208·715 | 66 | 722·69 | 21·7 | 564·14 | 6·4 | 90 | 6730 | 6820 | 1424 | 1440 | 1452 | SAUSSURE. |
| Môle (summit) | 199·855 | 53 | 723·03 | 19·7 | 565·00 | 4·0 | 5519 | 1300 | 6819 | 6853 | 6870 | 6291 | PICTET. |
| St. Jeoire | 208·40 | 62 | 724·54 | 17·9 | 565·73 | 5·3 | 352 | 6424 | 6776 | 1686 | 1746 | 1778 | KELLER. |
| Samoens | 207·94 | 55·5 | 727·61 | 17·0 | 566·91 | 2·5 | 747 | 6017 | 6764 | 2085 | 2153 | 2257 | WOEIL. |
| Col Dichaud | 198·24 | 52 | 727·73 | 23·9 | 568·46 | 7·3 | 6695 | 224 | 6919 | 8089 | 7946 | | |
| Servoz | 207·31 | 58 | 729·06 | 15·5 | 569·25 | 4·0 | 1171 | 5571 | 6742 | 2505 | 2599 | | |
| Chamounix—1. | 205·70 | 66·5 | 728·50 | 20·3 | 569·44 | 10·0 | 2174 | 4695 | 6869 | 3508 | 3475 | 3364 | SAUSSURE. |
| Chamounix—2. | 205·50 | 50·5 | 723·91 | 17·6 | 568·38 | 5·1 | 2076 | 4663 | 6739 | 3410 | 3507 | 3425 | FORBES. |
| Chamounix—3. | 205·52 | 68 | 721·40 | 15·4 | 566·11 | 9·2 | 1996 | 4751 | 6747 | 3330 | 3419 | 3408 | WOEIL. |
| Montanvert | 200·27 | 62 | 722·57 | 22·1 | 568·19 | 9·6 | 5286 | 1464 | 6750 | 6620 | 6706 | 6300 | FORBES. |
| Contamines—1. | 204·71 | 63 | 722·61 | 19·3 | 565·80 | 5·5 | 2544 | 4223 | 6767 | 3877 | 3947 | 4260 | WOEIL. |
| Contamines—2. | 204·72 | 62 | 722·41 | 15·4 | 564·33 | 6·5 | 2561 | 4305 | 6866 | 3896 | 3865 | 3259 | KELLER. |
| Croix du Bonhomme | 197·70 | 49 | 720·97 | 19·9 | 563·94 | 8·5 | 6694 | 131 | 6825 | 8028 | 8049 | 8026 | SAUSSURE. |
| Chapiu—1. | 202·35 | 54 | 721·67 | 15·5 | 563·55 | 3·4 | 3889 | 2883 | 6772 | 5223 | 5287 | 5071 | SAUSSURE. |
| Chapiu—2. | 202·655 | 52 | 724·52 | 17·5 | 563·02 | 1·2 | 3821 | 3069 | 6890 | 5155 | 5101 | 5006 | WOEIL. |
| Bourg St. Maurice | 207·00 | 59 | 726·97 | 16·5 | 565·59 | 1·2 | 1295 | 5534 | 6829 | 2629 | 2636 | | |
| Hospice Pt. St. Bernard | 199·24 | 50 | 724·47 | 29·9 | 566·64 | 9·0 | 5870 | 905 | 6775 | 7203 | 7265 | 7194 | SAUSSURE. |
| Cormayeur—1. | 204·75 | 57 | 723·74 | 16·0 | 565·69 | 4·3 | 2441 | 4336 | 6777 | 3775 | 3834 | 3997 | SAUSSURE. |
| Cormayeur—2. | 204·60 | 55·5 | 721·98 | 19·8 | 563·32 | 4·2 | 2535 | 4280 | 6815 | 3869 | 3890 | 4187 | FORBES. |
| Cormayeur—3. | 204·36 | 54 | 724·81 | 17·3 | 565·61 | 7·3 | 2806 | 3990 | 6796 | 4140 | 4180 | 4046 | WOEIL. |
| Aosta—1. | 208·59 | 73 | 724·19 | 19·4 | 565·72 | 8·7 | 225 | 6651 | 6876 | 1561 | 1519 | 1938 | SAUSSURE. |
| Aosta—2. | 208·46 | 61 | 721·74 | 14·2 | 561·61 | + ·2 | 205 | 6581 | 6786 | 1539 | 1589 | | |
| Aosta—3. | 208·35 | 68 | 722·23 | 15·9 | 561·26 | + 1·8 | 293 | 6611 | 6904 | 1627 | 1559 | | |
| Aosta—4. | 208·59 | 57 | 726·08 | 15·3 | 563·75 | − 2·1 | 293 | 6505 | 6798 | 1637 | 1685 | | |
| Aosta—5. | 208·56 | 72 | 726·41 | 20·4 | 567·18 | + 6·6 | 361 | 6542 | 6903 | 1695 | 1628 | | |
| St. Remy | 202·545 | 52 | 725·55 | 17·4 | 564·82 | + 0·6 | 3928 | 2913 | 6841 | 5262 | 5257 | 5263 | SAUSSURE. |
| Great St. Bernard—1. | 197·64 | 39 | 725·23 | 17·4 | 565·27 | + 2·7 | 6791 | ..... | 6791 | 8125 | ..... | 8170 | Bib. Univ. de Génève. |
| Great St. Bernard—2. | 197·68 | 38 | 726·42 | 17·1 | 566·67 | 6·5 | 6753 | ..... | 6753 | 8087 | ..... | 8038 | SAUSSURE. |
| St. Vincent—1. | 209·28 | 63 | 727·96 | 15·9 | 568·84 | 3·6 | − 54 | 6785 | 6731 | 1280 | 1385 | | |
| St. Vincent—2. | 209·64 | 55 | 730·25 | 20·0 | 569·04 | 2·8 | − 184 | 6799 | 6615 | 1150 | 1371 | | |
| Col de Jou | 202·82 | 56 | 730·25 | 21·0 | 569·99 | 5·8 | 3984 | 2871 | 6855 | 5318 | 5290 | | |
| Brussone | 204·45 | 66·5 | 730·25 | 22·8 | 570·41 | 7·3 | 2904 | 3740 | 6644 | 4238 | 4430 | 4431 | FORBES. |
| Col Ranzola | 199·45 | 53 | 728·99 | 24·3 | 570·59 | 7·9 | 6012 | 843 | 6855 | 7346 | 7327 | 7136 | FORBES. |
| Gressonay—1. | 204·06 | 58·5 | 730·52 | 18·0 | 570·94 | 4·3 | 3294 | 3681 | 6905 | 4558 | 4489 | 4208 | SAUSSURE. |
| Gressonay—2. | 204·33 | 58·5 | 730·15 | 18·4 | 570·29 | 6·1 | 3050 | 3766 | 6816 | 4384 | 4404 | 4526 | WOEIL. |
| Gressonay—3. | 203·92 | 57 | 730·37 | 16·9 | 568·27 | 2·6 | 3294 | 3752 | 7046 | 4628 | 4418 | | |
| Col Val Dobbia | 197·44 | 52 | 730·40 | 19·4 | 568·75 | 2·8 | 7226 | − 262 | 6964 | 8560 | 8432 | 7904 | SAUSSURE. |
| La Riva—1. | 205·54 | 61 | 729·94 | 15·8 | 569·74 | 2·5 | 2299 | 4494 | 6793 | 3633 | 3676 | 3568 | SAUSSURE. |
| La Riva—2. | 205·55 | 55 | 727·70 | 18·0 | 569·40 | 7·0 | 2200 | 4527 | 6727 | 3534 | 3643 | 3636 | WOEIL. |
| Varallo—1. | 209·36 | 67 | 728·18 | 20·0 | 570·86 | 7·4 | − 96 | 6814 | 6718 | 1238 | 1356 | | |
| Varallo—2. | 209·26 | 63 | 727·82 | 20·0 | 570·52 | 6·2 | − 47 | 6727 | 6680 | 1287 | 1443 | 1491 | WOEIL. |
| La Colma | 206·72 | 70 | 728·20 | 27·1 | 571·67 | 13·8 | 1560 | 5363 | 6923 | 2894 | 2907 | | |
| Orta—1. | 210·70 | 74 | 729·11 | 21·5 | 572·34 | 8·5 | − 895 | 7021 | 6726 | 439 | 549 | 1108 | WOEIL. |
| Orta—2. | 210·83 | 70 | 729·79 | 22·9 | 571·80 | 11·2 | − 921 | 7028 | 6707 | 387 | 518 | | |
| Orta—3. | 210·35 | 66 | 728·80 | 18·0 | 569·67 | 5·6 | − 681 | 7452 | 6771 | 653 | 718 | | |
| Mogadino—1. | 211·04 | 77 | 727·80 | 19·4 | 568·60 | 5·2 | −1146 | 7985 | 6839 | 188 | 185 | 678 | SAUSSURE. |
| Mogadino—2. | 211·20 | 68 | 724·55 | 18·0 | 566·74 | 6·0 | −1368 | 8110 | 6742 | − 34 | + 60 | | |
| Domo d'Ossola—1. | 210·68 | 78 | 733·38 | 17·4 | 565·64 | 5·9 | −1104 | 7933 | 6829 | 230 | 237 | 1004 | SAUSSURE. |
| Domo d'Ossola—2. | 210·44 | 66 | 733·27 | 16·0 | 563·89 | 4·5 | − 943 | 7751 | 6808 | 391 | 419 | | |
| Simplon Village | 203·28 | 57 | 724·03 | 17·0 | 564·12 | 7·4 | 3440 | 3441 | 6881 | 4729 | 4774 | 4792 | WOEIL. |
| Simplon Hospice | 201·07 | 48 | 724·00 | 19·0 | 564·13 | 5·3 | 4753 | 2084 | 6837 | 6087 | 6086 | 6580 | SAUSSURE. |
| Brieg—1. | 208·69 | 61 | 726·01 | 11·4 | 564·45 | + 1·6 | 229 | 6606 | 6835 | 1563 | 1564 | 2328 | SAUSSURE. |
| Brieg—2. | 208·35 | 56 | 729·28 | 15·0 | 566·00 | − 1·3 | 552 | 6194 | 6746 | 1886 | 1976 | 2183 | WOEIL. |
| Leukerbad—1. | 204·38 | 47·5 | 733·42 | 14·6 | 570·43 | − 1·2 | 3104 | 3673 | 6777 | 4438 | 4497 | 4699 | WOEIL. |
| Leukerbad—2. | 204·545 | 45 | 734·75 | 13·7 | 571·54 | + 2·0 | 3025 | 3749 | 6774 | 4359 | 4421 | | |
| S. Crest of the Gemmi | 199·335 | 41 | 734·44 | 19·7 | 572·06 | 4·7 | 6235 | 620 | 6855 | 7569 | 7550 | 7626 | WOEIL. |
| Kandersteg—1. | 206·34 | 55·5 | 733·06 | 16·3 | 572·45 | 3·0 | 1922 | 4816 | 6738 | 3256 | 3354 | 3493 | WOEIL. |
| Kandersteg—2. | 206·17 | 48 | 732·09 | 15·8 | 571·74 | 9·3 | 1972 | 4765 | 6737 | 3306 | 3405 | | |
| Lauterbrunnen—1. | 208·28 | 55 | 728·81 | 16·6 | 570·66 | 8·8 | 585 | 6112 | 6697 | 1918 | 1958 | | |
| Lauterbrunnen—2. | 208·37 | 49 | 727·68 | 17·6 | 569·18 | 10·3 | 484 | 6215 | 6699 | 1818 | 1955 | | |
| Bern | 209·26 | 72 | 726·97 | 22·4 | 569·91 | 11·0 | − 82 | 6883 | 6801 | 1252 | 1287 | 1661 | WOEIL. |

The discrepancies in many of the above results as compared with the column of previously determined heights are considerable, but it is to be observed that in most cases where the height of the station may be considered as particularly well-established, the observed height accords with it in a very remarkable manner. This is particularly the case at Geneva (where the height is of course deduced only from that of the Great St. Bernard), at Bonneville, at Chamounix (taking a mean of the six observations, which gives 3442), at the Croix du Bonhomme*, at St. Remy, at the Great St. Bernard and at Brussone: the chief exceptions being at the Môle†, the Montanvert, the Col di Val Dobbia, Orta, Mogadino, Domo d'Ossola, the Simplon Hospice and Brieg, the great differences at which places are to me quite unaccountable, unless on the supposition, in the case of the five last-named, that their distances from the fixed stations were too great to allow of trustworthy results being obtained from single observations. In confirmation of this view, it will be seen presently that these observations give far more satisfactory results when employed without any reference to the simultaneous observations at Geneva and the Great St. Bernard; and also in the above Table we may notice that, after crossing the Bonhomme, in those cases where the heights determined from the two fixed stations differ from one another to any considerable amount, that obtained by reference to the Great St. Bernard generally approximates more nearly to the height as previously determined than the other, obtained by a comparison with the more distant station.

The numbers in the tenth column which are the sums of the two values, the height above Geneva and the depression below the Great St. Bernard, as determined by observation, should, supposing both theory and observations to be perfect, give the constant value 6836, which is the difference of height of those two stations; but this difference will vary when determined by means of each pair of barometric observations, the standard value 6836 being the mean of a very considerable number. Now it is very evident that were it not for that term in the formula which involves the correction for the expansion of the air, the sum of the above values would give the difference of altitude, however erroneous the intermediate observation might be: for calling $b'$, $b''$ and $b'''$ the three boiling-points, we should have

$$548(b'-b'')-24D+548(b''-b''')+24D=548(b'-b''').$$

The observation, therefore, of the external temperature at the intermediate station becomes the means of rendering the accordance of the sums of the component altitudes with the whole height between Geneva and the Great St. Bernard, as determined in each case, a test of the correctness of the observation of the boiling temperature. We may in fact, by equating the formula for the whole height with the sum

* All who have traversed the Pass of the Bonhomme are aware that in crossing to the south, upon Chapiu, the tract rises considerably after passing what ought to be and, I believe, *is* considered the "*Col*," viz. the point from which a view of the mountains of the Tarentaise is first obtained; this point is marked by a cross, larger than usual on most passes, and than any of the numerous others on this pass. This cross I believe to be the "*Croix du Bonhomme*" of De Saussure, and it was at its base the observation above referred to was made.

† See note, p. 131.

of those expressing the two component heights, arrive at an expression for the intermediate boiling-point in terms of the atmospheric temperatures at the three stations and the upper and lower boiling-points; such an expression is

$$b'' = \frac{23}{24} \cdot \left\{ \frac{t - T'}{T - T'} \cdot b' + \frac{T - t}{T - T'} b'' + 8{\cdot}4 \right\}$$

where T, $t$ and T′ are the temperatures at the stations where the boiling-points are respectively $b'$, $b''$ and $b'''$; but this, when put in the form

$$b'' = \frac{23}{24} \cdot \left\{ \frac{t.(b' - b''') + Tb''' - T'b'}{T - T'} + 8{\cdot}4 \right\},$$

shows us that an error in the value of $t$, of trifling moment where that value is to be employed in the determination of the difference of level, will cause an error in the resulting boiling temperature of very serious amount when used for the same purpose. Though this test cannot therefore be made use of in its full rigour, it will be highly satisfactory if an accordance can be shown between the results above referred to: the following, Table II., exhibits a comparison of these values. The numbers in the second column are the differences of the boiling temperatures, in degrees of FAHRENHEIT, deduced from the barometric observations at Geneva and the Great St. Bernard, which were simultaneous with those of my own made at the places named in the first column.

TABLE II.

| Station. | Difference of boiling-points derived from the observations at Geneva and the Great St. Bernard. | Value of $(t-32°)$ in formula II. | Derived difference of height. | Difference of height from Table I. | Station. | Difference of boiling-points derived from the observations at Geneva and the Great St. Bernard. | Value of $(t-32°)$ in formula II. | Derived difference of height. | Difference of height from Table I. |
|---|---|---|---|---|---|---|---|---|---|
| Les Rousses | 11·583 | 22·8 | 6669 | 6668 | Col de Jou | 11·860 | 24·1 | 6847 | 6855 |
| Geneva | | | | 6792 | Brussone | 11·372 | 27·0 | 6605 | 6644 |
| Bonneville | 11·856 | 25·3 | 6862 | 6830 | Col Ranzola | 11·716 | 29·0 | 6833 | 6855 |
| Môle | 11·794 | 21·4 | 6768 | 6810 | Gressonay—1. | 11·789 | 20·0 | 6719 | 6905 |
| St. Jeoire | 11·832 | 20·9 | 6784 | 6776 | Gressonay—2. | 11·818 | 22·0 | 6763 | 6816 |
| Samoens | 11·936 | 17·6 | 6796 | 6764 | Gressonay—3. | 12·003 | 17·5 | 6833 | 7046 |
| Col Dichaud | 11·813 | 28·1 | 6877 | 6919 | Col di Val Dobbia | 11·964 | 20·0 | 6847 | 6964 |
| Servoz | 11·834 | 12·6 | 6666 | 6742 | La Riva—1. | 11·851 | 16·5 | 6735 | 6793 |
| Chamounix—1. | 11·781 | 27·3 | 6846 | 6869 | La Riva—2. | 11·729 | 22·5 | 6749 | 6727 |
| Chamounix—2. | 11·568 | 20·5 | 6627 | 6739 | Varallo—1. | 11·640 | 24·7 | 6728 | 6718 |
| Chamounix—3. | 11·593 | 22·2 | 6664 | 6747 | Varallo—2. | 11·646 | 23·6 | 6716 | 6680 |
| Montanvert | 11·491 | 28·6 | 6696 | 6750 | La Colma | 11·574 | 26·8 | 6860 | 6823 |
| Contamines—1. | 11·699 | 23·3 | 6738 | 6767 | Orta—1. | 11·578 | 27·0 | 6735 | 6726 |
| Contamines—2. | 11·811 | 19·7 | 6755 | 6866 | Orta—2. | 11·661 | 25·7 | 6758 | 6707 |
| Croix du Bonhomme | 11·748 | 25·6 | 6803 | 6825 | Orta—3. | 11·782 | 21·3 | 6761 | 6771 |
| Chapiu—1. | 11·828 | 17·0 | 6736 | 6772 | Mogadino—1. | 11·805 | 22·1 | 6786 | 6839 |
| Chapiu—2. | 12·061 | 16·9 | 6857 | 6880 | Mogadino—2. | 11·748 | 21·6 | 6746 | 6742 |
| Bourg St. Maurice | 12·005 | 16·0 | 6811 | 6829 | Domo d'Ossola—1. | 11·764 | 20·9 | 6746 | 6829 |
| Hospice Pt. St. Bernard | 11·642 | 28·7 | 6786 | 6775 | Domo d'Ossola—2. | 11·865 | 18·5 | 6762 | 6808 |
| Cormayeur—1. | 11·748 | 25·6 | 6803 | 6777 | Simplon Village | 11·936 | 22·0 | 6859 | 6881 |
| Cormayeur—2. | 11·953 | 15·4 | 6775 | 6815 | Simplon Hospice | 11·933 | 20·9 | 6842 | 6837 |
| Cormayeur—3. | 11·861 | 27·1 | 6852 | 6796 | Brieg—1. | 12·038 | 11·7 | 6768 | 6835 |
| Aosta—1. | 11·809 | 25·3 | 6835 | 6876 | Brieg—2. | 12·121 | 12·4 | 6826 | 6746 |
| Aosta—2. | 11·997 | 13·0 | 6785 | 6786 | Leukerbad—1. | 12·020 | 19·1 | 6763 | 6777 |
| Aosta—3. | 12·060 | 15·9 | 6842 | 6904 | Leukerbad—2. | 12·014 | 14·2 | 6790 | 6774 |
| Aosta—4. | 12·102 | 13·2 | 6806 | 6798 | South Crest of the Gemmi | 11·950 | 22·0 | 6868 | 6855 |
| Aosta—5. | 11·895 | 24·3 | 6835 | 6903 | Kandersteg—1. | 11·928 | 17·4 | 6731 | 6738 |
| St. Remy | 11·976 | 16·2 | 6798 | 6841 | Kandersteg—2. | 11·823 | 22·6 | 6804 | 6737 |
| Great St. Bernard—1. | 11·911 | 18·1 | 6790 | 6791 | Lauterbrunnen—1. | 11·700 | 22·9 | 6737 | 6697 |
| Great St. Bernard—2. | 11·873 | 21·3 | 6816 | 6753 | Lauterbrunnen—2. | 11·749 | 25·1 | 6797 | 6699 |
| St. Vincent—1. | 11·795 | 16·5 | 6715 | 6731 | Bern | 11·641 | 30·1 | 6805 | 6801 |
| St. Vincent—2. | 11·930 | 20·5 | 6835 | 6615 | | | | | |

It is to be observed, that the differences of height determined by the two methods do not differ from one another in any single case by so large a quantity as do the greatest and least difference of level in the fourth column, determined from the barometric observations at the two places; while in many the accordance is almost perfect. One of the most satisfactory results in this respect is, however, that derived from the observation No. 2. at Mogadino, which appears from Table I. to be one of those least likely to be correct, since it places the surface of the Lago Maggiore much below its possible level. This alone sufficiently shows that the accordance above remarked can only be taken as a general indication of the accuracy of the instrument.

Having thus shown that the instrument is perfectly capable of furnishing correct data for the determination of difference of level, though apparently its indications cannot in all cases be relied on, it becomes further interesting to inquire how far it may be depended on when made use of without reference to corresponding observations at any fixed station. It is obvious that the accuracy of a series of heights thus obtained must, to a certain extent, be vitiated by the diurnal changes in the atmospheric pressure, but it will be seen that this source of error does not operate to the extent that might, *à priori*, be supposed probable, and that, whether from a fortuitous balancing of errors, or from some other cause, the discrepancies in those cases where the previously determined height is probably most near the truth, are less considerable than might be expected. In order thus to obtain the results given in the following Table III., it became necessary again to modify the standard formula on account of the instrumental error. By multiplying the correction, C=·0433, by 548 we obtain it in the form of a difference of altitude for each degree of observed difference of boiling temperature at two successive places of observation, to be added to the factor 548 to form the constant multiplier for this particular instrument. We thus obtain

$$548 \times 1{\cdot}0433 = 571{\cdot}72,$$

or, with sufficient accuracy, 572.

The working formula then becomes

$$H = 572(b - b').\{1 + (t - 32°)\cdot 00222\}.$$

In the following Table, in which the results have been calculated with this formula, the differences of level are determined from station to station in the order in which the observations were made, and, in case of two or more observations having been made at the same station, those most approximate in time have been employed. Taking the station at Geneva as a starting-point, and assuming its level, derived from that of the surface of the lake given in the Bibliothèque Universelle de Génève, to be 1264 English feet above the sea level, the calculated differences have been added to this in succession, and the altitudes by observation obtained.

## Table III.

| Station. | Time of observation, 1844. | External temperature of the air. | Observed boiling-point. | Dif. of altitude in ft. | Altitude in feet. | Altitude as previously determined. | Authority for the altitudes in the preceding column. |
|---|---|---|---|---|---|---|---|
| | h m | ° | | | | | |
| Les Rousses (Jura) | June 25. 5 30 A.M. | 57 | 204·48 | 2767 | 4031 | 3919 | Woerl. |
| Geneva—1. | 25. 0 30 P.M. | 71 | 208·82 | 2767 | .... | 1264 | Bib. Univ. de Genève. |
| Geneva—2. | 26. 6 15 A.M. | 62 | 209·115 | | | | |
| Bonneville | 26. 11 45 A.M. | 66 | 208·715 | 247 | 1511 | 1452 | Saussure. |
| Môle | 26. 6 15 P.M. | 53 | 199·855 | 5388 | 6899 | 6094<br>6221 | Saussure.<br>Pictet. |
| St. Jeoire | 27. 8 0 A.M. | 62 | 208·40 | 5166 | 1733 | 1893 | De Luc. |
| Samoens | 28. 5 30 A.M. | 55·5 | 207·94 | 278 | 2011 | 2270 | De Luc. |
| Col Dichaud (Buet) | 28. 3 45 P.M. | 52 | 198·24 | 5816 | 7827 | | |
| Servoz | 29. 7 30 A.M. | 58 | 207·31 | 5453 | 2374 | | |
| Chamounix—1. | 29. 1 45 P.M. | 66·5 | 205·70 | 983 | 3357 | 3364 | Saussure. |
| Chamounix—2. | 30. 4 30 A.M. | 50·5 | 205·50 | .... | .... | 3425 | Forbes. |
| Montanvert | 30. 0 15 P.M. | 62 | 200·27 | 3152 | 6509 | 6101<br>6300 | Saussure.<br>Forbes. |
| Chamounix—3. | 30. 4 20 P.M. | 68 | 205·52 | 3223 | 3286 | 3356 | Pictet. |
| Contamines | July 1. 7 15 P.M. | 63 | 204·71 | 500 | 3786 | 3328 | Keller. |
| | 2. 9 15 A.M. | 62 | 204·72 | .... | .... | 4260 | Woerl. |
| Croix du Bonhomme | 2. 3 30 P.M. | 49 | 197·70 | 4269 | 8055 | 8026 | Saussure. |
| Chapiu—1. | 2. 7 30 P.M. | 54 | 202·35 | 2775 | 5280 | 5071 | Saussure. |
| Chapiu—2. | 3. 9 20 A.M. | 52 | 202·655 | .... | .... | 4974 | Pictet. |
| Bourg St. Maurice | 3. 8 0 P.M. | 59 | 207·00 | 2566 | 2714 | | |
| Petit St. Bernard | 4. 11 45 A.M. | 50 | 199·24 | 4660 | 7374 | 7194 | Saussure. |
| Cormayeur—1. | 4. 9 30 P.M. | 57 | 204·75 | 3302 | 4072 | 3997 | Saussure. |
| Cormayeur—2. | 7. 7 0 A.M. | 54 | 204·36 | .... | .... | 4167 | Forbes. |
| Aosta—1. | 7. 1 45 P.M. | 73 | 208·59 | 2589 | 1483 | 1938 | Saussure. |
| Aosta—2. | 9. 5 30 A.M. | 57 | 208·59 | .... | .... | 1980 | Woerl. |
| St. Remy | 9. 11 45 A.M. | 52 | 202·545 | 3631 | 5114 | 5263 | Saussure. |
| Grand St. Bernard—1. | 9. 3 30 P.M. | 39 | 197·640 | 2891 | 8003 | 8038 | Saussure. |
| Grand St. Bernard—2. | 10. 6 30 A.M. | 38 | 197·680 | .... | .... | 8170 | Bib. Univ. de Genève. |
| Aosta—3. | 10. 0 30 P.M. | 72 | 208·560 | 6541 | 1462 | | |
| St. Vincent—1. | 10. 9 0 P.M. | 63 | 209·280 | 444 | 1018 | | |
| St. Vincent—2. | 11. 5 30 A.M. | 55 | 209·64 | | | | |
| Col de Jou | 11. 10 30 A.M. | 56 | 202·82 | 4148 | 5166 | | |
| Brussone | 11. 0 0 noon. | 66·5 | 204·65 | 1109 | 4057 | 4431 | Forbes. |
| Col de Ranzola | 11. 5 30 P.M. | 53 | 199·45 | 3157 | 7214 | 7136 | Forbes. |
| Gressonay—1. | 11. 7 45 P.M. | 58·5 | 204·06 | 2776 | 4438 | 4208 | Saussure. |
| Gressonay—2. | 13. 8 15 A.M. | 57 | 203·92 | .... | .... | 4526 | Woerl. |
| Col di Val Dobbia | 13. 1 0 P.M. | 52 | 197·44 | 3892 | 8330 | 7904 | Saussure. |
| La Riva—1. | 13. 7 30 P.M. | 61 | 205·54 | 4885 | 3445 | 3568 | Saussure. |
| La Riva—2. | 14. 6 0 A.M. | 55 | 205·55 | .... | .... | 3712 | Woerl. |
| Varallo—1. | 14. 10 0 P.M. | 67 | 209·36 | 2319 | 1126 | 1491 | Woerl. |
| Varallo—2. | 15. 6 45 A.M. | 63 | 209·26 | | | | |
| La Colma | 15. 2 15 P.M. | 70 | 206·72 | 1566 | 2692 | | |
| Orta—1. | 15. 6 45 P.M. | 74 | 209·70 | 1856 | 836 | 1108 | Woerl. |
| Orta—2. | 17. 4 30 A.M. | 66 | 210·35 | | | | |
| Mogadipo—1. | 17. 6 30 P.M. | 77 | 211·04 | 430 | 406 | 678 | Saussure. |
| Mogadino—2. | 18. 6 0 A.M. | 68 | 211·20 | | | | |
| Domo d'Ossola—1. | 18. 7 0 P.M. | 78 | 210·68 | 324 | 735 | 1004 | Saussure. |
| Domo d'Ossola—2. | 19. 4 30 A.M. | 66 | 210·44 | | | | |
| Simplon (Village) | 19. 0 30 P.M. | 57 | 203·28 | 4368 | 5098 | 4792 | Woerl. |
| Simplon Hospice | 19. 4 50 P.M. | 48 | 201·07 | 1323 | 6421 | 6580 | Saussure. |
| Brieg—1. | 19. 10 30 P.M. | 61 | 208·69 | 4581 | 1840 | 2328 | Saussure. |
| Brieg—2. | 20. 7 15 A.M. | 56 | 208·35 | .... | .... | 2183 | Woerl. |
| Leukerbad—1. | 20. 8 0 P.M. | 47·5 | 204·38 | 2371 | 4211 | 4697 | Woerl. |
| Leukerbad—2. | 21. 7 0 A.M. | 45 | 204·545 | | | | |
| Gemmi (châlet on south crest) | 21. 0 30 P.M. | 41 | 199·235 | 3112 | 7323 | 7626 | Woerl. |
| Kandersteg—1. | 21. 6 30 P.M. | 55·5 | 206·34 | 4208 | 3115 | 3493 | Woerl. |
| Kandersteg—2. | 22. 6 45 A.M. | 48 | 206·17 | | | | |
| Lauterbrunnen—1. | 22. 9 0 P.M. | 55 | 208·28 | 1251 | 1864 | | |
| Lauterbrunnen—2. | 23. 6 45 A.M. | 49 | 208·37 | | | | |
| Bern | 24. 10 20 A.M. | 72 | 209·26 | 536 | 1328 | 1661 | Woerl. |

Though the discrepancies in these results, when compared with the previously determined altitudes, appear very considerable, they are probably in most cases not more so than would have resulted from the employment of the usual form of barometer. I am led to this conclusion from the inspection of De Luc's series of barometric measurements, wherein the determinations of the height of the same station at different times frequently differ from one another 250 feet, and, in one case, as much as 360 feet. The greatest error in the above table, viz. that at Aosta*, 400 feet, certainly exceeds either of these, but there is, I think, good reason to suppose that the observed height at Aosta is not so erroneous as the comparison with De Saussure's result makes it appear to be, when we remark that the height of the Great St. Bernard determined from it is one of the most accurate of the series, and also that the observations made *the next day* at the Great St. Bernard and at Aosta give the same difference of level, within twenty feet.

The only other instance in which I was enabled to repeat an observation after the ascent of any considerable height occurred at Chamounix, and in it a tolerably close accordance in the observations before and after the ascent of the Montanvert is also to be remarked, though the height derived from the intermediate observation differs very considerably even from that given by Professor Forbes, which is however equally in excess above that by De Saussure.

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Since the calculation of the above results I have had an opportunity of comparing the instrument with a standard barometer, and have been surprised to find the instrumental correction, under the pressure 30 inches, very different from that obtained above from the Geneva observations. I cannot assign any reason for supposing an error in making these observations; indeed, every consideration induces me to believe that no such error could have occurred, but at the same time I find it very difficult to account for this change in the correction, unless it be partly attributable to a difference in construction of the two barometers and partly to a difference in the degree of purity of the water, or to a change in the instrument itself; all of which suppositions are highly improbable. To whichever of the above causes this remarkable change is referred, I do not think it should be considered as vitiating the results derived from the recorded observations, since they have been connected with those made with the barometers by means of which the correction made use of has been determined; and moreover the time elapsed between the first and last recorded observations was only one month, and the sources from which the water was derived were in all cases so far similar as apparently to ensure its chemical identity with respect to boiling temperature.

The utility of an instrument to be applied to purposes which involve the necessity

* The circumstances of danger under which the observation on the Môle was made, make me think that an erroneous reading is not unlikely to have occurred there, and I accordingly exclude it from consideration in these remarks.

of its removal from place to place under difficult circumstances, is not *exclusively* to be measured by the degree of delicacy or even of accuracy of its indications; since however perfect it may be theoretically, when stationary, it can never be of practical benefit unless it be of such a construction as to bear the concussions and shaking it must be necessarily exposed to when conveyed in the manner in which it can alone arrive at the point where its agency is required. This construction has been attained in the instruments by means of which are determined the two principal coordinates of geographical position, under all circumstances involving difficulty of access to the place of observation, while the third, *height above the sea level*, has had almost exclusively applied to its determination an instrument more liable than almost any other to suffer during removal, and, from its very nature, incapable of any great improvement in this respect. With regard to the instrument under consideration, I can, in reference to the above points, speak most positively; not only were the concussions which it constantly suffered very considerable, but on two or three occasions it escaped uninjured by blows which must have shivered any common barometer, however externally protected; and it has only at length been destroyed through having been inadvertently left boiling;—the water having evaporated, the increase of temperature of the empty boiler proved too great for the capacity of the tube of the thermometer, and the bulb burst.

Since writing the above my attention has been directed to the researches of Holtzmann and Magnus, given in Vol. iv. Part 14. of the "Scientific Memoirs," and I have applied the formula for the expansive force of steam deduced by the former, to the determination of an expression for the difference of level in terms of the difference of boiling temperature. Reduced to English measures, Holtzmann's result gives

$$b-32^\circ=425\cdot2\times\frac{\log\beta+\cdot74883}{6\cdot73157-\log\beta}\quad\text{III.}$$

$b$ and $\beta$ being the same as in (I.).

Applying this as before, in the case of De Luc's, I obtain

$$H=\frac{191939000}{(393\cdot2+b)(393\cdot2+b')}\cdot(b-b')\{1+(t-32^\circ)\cdot00222\}\quad\text{IV.}$$

The results derived from (III.), within the limits of my observations, differ but little from those obtained from (I.), but have a tendency to diminish the boiling temperature more rapidly as the pressure decreases, as the following results sufficiently show:—

| Pressure. | Temperature. | | |
|---|---|---|---|
| | De Luc. | Holtzmann. | Diff. |
| 721·74 mm. | 209·386 | 209·425 | +·039 |
| 561·61 mm. | 197·389 | 197·237 | −·152 |

Even were this last difference greater, we must still rely upon De Luc's until it can be shown that the results obtained with (III.) are more nearly in accordance with observations made under natural atmospheric pressures than those derived from (I.) have been found to be.

The practical disadvantages of (IV.) are evident.

## X. *Investigation of an Extensive Class of Partial Differential Equations of the Second Order, in which the Equation of* LAPLACE'S *Functions is included.*

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*Theorem.* IF $u$ be a function of $x$ and $y$ satisfying the equation

$$\frac{d^2u}{dxdy}+\alpha_n e^{\phi}u=0,$$

where

$$\frac{d^2\phi}{dxdy}+ce^{\phi}=0,$$

then the solution will be

$$u=\mathrm{D}^{-n}v_n$$

where

$$\mathrm{D}=e^{-\phi}\frac{d}{dy},$$

where

$$v_n=\int e^{-\frac{\beta_n}{\Delta\beta_n}\phi}\chi y\,dy+\psi x,$$

$\chi y$ and $\psi x$ arbitrary functions of $y$ and $x$,

and

$$\beta_n=\frac{\Delta\alpha_{n-1}.\Delta\alpha_{n-2}.\ldots.}{(\Delta\alpha_{n-1}-c)(\Delta\alpha_{n-2}-c)\ldots.},$$

where

$$\Delta\alpha_r=\alpha_{r+1}-\alpha_r,$$

and $\alpha_n$ is a function of $n$ vanishing for $n=0$ and for $n=-1$.

I will proceed to demonstrate this curious theorem as briefly as possible.

According to the notation, we may write the given equation

$$\mathrm{D}\frac{du}{dx}+\alpha_n u=0.$$

Then if

$$v_n=\mathrm{D}^n u$$

we have

$$v_{n+1}=\mathrm{D}^{n+1}u=\mathrm{D}v_n.$$

Suppose that

$$\mathrm{D}\frac{dv_n}{dx}+\beta_n z\mathrm{D}v_n=\mathrm{D}^n\left\{\mathrm{D}\frac{du}{dx}+\alpha_n u\right\}. \quad . \quad . \quad . \quad . \quad . \quad (\alpha.)$$

where $z$ is a function of $x$ and $y$ to be determined, also $\beta_n$ a function of $u$.

Writing $n+1$ for $n$ in equation ($\alpha.$), we ought to have

$$\mathrm{D}\frac{dv_{n+1}}{dx}+\beta_{n+1}z\mathrm{D}v_{n+1}=\mathrm{D}^{n+1}\left\{\mathrm{D}\frac{du}{dx}+\alpha_{n+1}u\right\} \quad . \quad . \quad . \quad . \quad . \quad (\beta.)$$

This circumstance will serve to determine $z$ and $\beta_n$ as follows: we have identically

$$D^{n+1}\left\{D\frac{du}{dx}+\alpha_{n+1}u\right\}=D^{n+1}\left\{D\frac{du}{dx}+\alpha_n u\right\}+\Delta\alpha_n D^{n+1}u$$

$$=D^2\frac{dv_n}{dx}+\beta_n D\left\{zDv_n\right\}+\Delta\alpha_n . Dv_n \text{ by } (\alpha)$$

$$=D^2\frac{dv_n}{dx}+\beta_n D\left\{zv_{n+1}\right\}+\Delta\alpha_n . v_{n+1}.$$

But

$$\frac{dv_{n+1}}{dx}=\frac{d}{dx}Dv_n=\frac{d}{dx}\left(e^{-\varphi}\frac{dv_n}{dy}\right)$$

$$=-\frac{d\varphi}{dx}e^{-\varphi}\frac{dv_n}{dy}+e^{-\varphi}\frac{d}{dy}\cdot\frac{dv_n}{dx}$$

$$=-\frac{d\varphi}{dx}Dv_n+D\frac{dv_n}{dx};$$

$$\therefore\quad D^2\frac{dv_n}{dx}=D\left\{\frac{dv_{n+1}}{dx}+\frac{d\varphi}{dx}v_{n+1}\right\}.$$

Hence

$$D\frac{dv_{n+1}}{dx}+D\left(\frac{d\varphi}{dx}v_{n+1}\right)+\beta_n D(zv_{n+1})+\Delta\alpha_n v_{n+1}$$

ought to be identical with $D\frac{dv_{n+1}}{dx}+\beta_{n+1}zDv_{n+1}$, and hence the conditions

$$\frac{d\varphi}{dx}+\beta_n z=\beta_{n+1}z$$

$$D\left\{\frac{d\varphi}{dx}+\beta_n z\right\}=-\Delta\alpha_n.$$

Eliminating $z$, we have

$$D\frac{d\varphi}{dx}=-\frac{\Delta\alpha_n\Delta\beta_n}{\beta_{n+1}}=-c,$$

or

$$\frac{d^2\varphi}{dxdy}+ce^{\varphi}=0,$$

and

$$c\beta_{n+1}=\Delta\alpha_n\{\beta_{n+1}-\beta_n\},$$

or

$$\beta_{n+1}=\frac{\Delta\alpha_n}{\Delta\alpha_n-c}\cdot\beta_n;$$

$$\therefore\quad \beta_n=\frac{\Delta\alpha_{n-1}.\Delta\alpha_{n-2}\ldots.}{(\Delta\alpha_{n-1}-c)(\Delta\alpha_{n-2}-c)\ldots.};$$

by these determinations we establish the formula $(\beta.)$ as a consequence of $(\alpha.)$, and therefore if the formula $(\alpha.)$ be true for any value of $n$, it will be (subject to the above conditions) true for the next superior value.

Now, when $n=0$, $v_0=D^0u=u$, and provided $\alpha_0$ and $\alpha_{-1}$ are each $=0$, $\alpha_0$ and $\Delta\alpha_{-1}$ will be each 0, and $\therefore$ $\alpha_0$ and $\beta_0$ each $=0$, and the equation $(\alpha.)$ reduces to $D\frac{dv_0}{dx}$

$=D\frac{du}{dx}$, and is therefore true for $u=0$. Under these restrictions it will therefore be true for any positive integral value of $n$. Now the symbol D represents $e^{-\phi}\frac{d}{dy}$, and therefore if $U=0$, $D^nU=0$, so that we have

$$D\frac{dv_n}{dx}+\beta_n x Dv_n=0,$$

or
$$e^{-\phi}\frac{d^2v_n}{dxdy}+\frac{\beta_n}{\Delta\beta_n}\cdot\frac{d\varphi}{dx}\cdot e^{-\phi}\frac{dv_n}{dy}=0,$$

or
$$\frac{\frac{d}{dx}\cdot\frac{dv_n}{dy}}{\frac{dv_n}{dy}}=-\frac{\beta_n}{\Delta\beta_n}\cdot\frac{d\varphi}{dx};$$

$\therefore$ integrating with respect to $x$,

$$\frac{dv_n}{dy}=e^{-\frac{\beta_n}{\Delta\beta_n}\phi}\chi y$$

$$v_n=\int e^{-\frac{\beta_n}{\Delta\beta_n}\phi}\chi y dy+\downarrow x,$$

and
$$u=D^{-n}v_n=\int e^{\phi}\int e^{\phi}\ldots\ldots v_n dy dy\ldots.$$

the integral sign repeated $n$ times. The theorem is therefore demonstrated.

It may be easily shown that the equation of LAPLACE's coefficients is included in the class here considered.

The equation of LAPLACE by a proper choice of independent variables assumes the form

$$\frac{d^2u}{dxdy}+\frac{n.n+1}{4\cos^2\frac{y-x}{2}}\cdot u=0.$$

Hence with reference to the preceding investigation,

$$D=\cos^2\frac{y-x}{2}\cdot\frac{d}{dy}\text{ and }\alpha_n=\frac{n.\overline{n+1}}{4}.$$

Hence
$$e^{-\phi}=\cos^2\frac{y-x}{2};$$

$\therefore$
$$\frac{d\varphi}{dx}=-\tan\frac{y-x}{2}$$

$$\frac{d^2\varphi}{dxdy}+\frac{1}{2}e^{\phi}=0.$$

Hence
$$c=\frac{1}{2}.\quad\text{Also }\Delta\alpha_n=\frac{n+1}{2};\quad\Delta\alpha_n-c=\frac{n}{2};$$

$\therefore$
$$\beta_n=n\text{ and }\Delta\beta_n=1.$$

Inserting these values in the final formula, we have

$$v_n = \int \cos^{2n} \frac{y-x}{2} \chi y dy + \downarrow x,$$

and $$u = \int \cos^{-2} \frac{y-x}{2} \int \cos^{-2} \frac{y-x}{2} \ldots . v_n dy dy \ldots . n \text{ times},$$

which agrees with Mr. HARGREAVE's solution.

## XI. *On Spontaneous Nitrification.* *By* C. F. Schoenbein, *Professor of Chemistry in the University of Bâle.* *Communicated by* Michael Faraday, *Esq., F.R.S., &c.*

Received November 6, 1845,—Read February 12, 1846.

A FACT not yet well understood is the spontaneous formation of nitric acid, or rather nitrates which takes place when azotic organic matters putrify in contact with strong bases and atmospheric air. Some chemists hold the opinion that under the circumstances mentioned, nitric acid is formed out of the nitrogen of the organic substances and atmospheric oxygen, both elements being determined to unite with one another by what is called the predisposing affinity of the bases for nitric acid.

Others maintain that the said acid originates entirely from the oxygen and nitrogen of the atmosphere, and suppose that these elements are by a sort of capillary action brought into such a condensed state as to combine into nitric acid, under the additional influence of the predisposing affinity of the bases for that acid.

According to the notions prevailing at present nitric acid comes from ammonia, which is supposed to be acted upon by the oxygen of the air, *i. e.* dehydrogenized, and its nitrogen united to oxygen. The only thing sure is, that we know very little about spontaneous nitrification.

Considering the theoretical interest connected with the obscure phenomenon in question, it must appear very desirable that novel facts should be ascertained calculated to throw light upon, and lead to the true insight into, the cause of spontaneous nitrification. The observations I am about to state will perhaps contribute a little to obtain that end.

Some years ago I ascertained the fact, that a solution of potash, sufficiently long treated with air ozonized (by the means of phosphorus), yields a liquid which, being over-saturated with sulphuric acid and heated, causes a brownish coloration round a crystal of sulphate of iron, and has the power of destroying indigo*. From that fact I inferred at the time that the reactions indicated are due to the presence of a nitrate.

Some time after I found out that the acid liquid called phosphatic acid, produced during the action of phosphorus upon moist atmospheric air, enjoys likewise the property of destroying indigo. If that acid is perceptibly coloured by a solution of indigo and left to itself, after a few hours it will have become colourless, or if the same acid be heated, its colour will be discharged within a few minutes.

Phosphatic acid, some iodide of potassium being dissolved in it, assumes a yellowish

* See Les Archives de l'Electricité, No. 15, p. 417.

brown colour, and the same acid being neutralized by hydrate of lime, yields in small quantities a salt exhibiting all the properties of nitrate of lime. The latter being treated, for instance, with carbonate of ammonia, produces a salt which, when mixed with some strong sulphuric acid and brucine, strikes a blood-red colour, disengages fumes of hyponitric acid, &c. From these facts it appears that nitric acid is present in the phosphatic acid, the former being produced during the oxidation which phosphorus undergoes in moist atmospheric air.

I have further ascertained that hydrate of lime, being suspended in water and sufficiently long treated with ozonized air, produces a salt exhibiting the properties of a nitrate.

M. Fellenberg has published* results which lead also to the conclusion, that either nitrites or nitrates are formed in treating hydrate of lime with ozonized atmospheric air, and the same chemist has made observations with the phosphatic acid coinciding with the results I have obtained from my own experiments. The philosopher of Lausanne is rather inclined to identify ozone with hyponitric acid. I think, however, that such an opinion cannot be upheld, and is contrary to facts, of which I gave an account in Poggendorff's Annals some time ago. According to my experiments, ozone destroys instantaneously hyponitric acid, hydrate of nitric acid being formed. Phosphorus does not shine in atmospheric air containing the smallest quantity of hyponitric acid, whilst phosphorus exhibits a very lively disengagement of light when placed in atmospheric air, being ever so strongly charged with ozone. Blue litmus paper being suspended in air of the latter description is not in the least reddened, but simply and rapidly bleached; whilst the same paper placed in atmospheric air, which is only slightly charged with hyponitric acid, assumes a perceptibly red colour. Ozonized air being charged with vapour of ammonia or carbonate of ammonia so strongly as to bring back instantly the blue colour of reddened litmus paper, enjoys the property of decomposing iodide of potassium (*i. e.* colouring blue the parts of starch mixed with that iodide), transforming the yellow prussiate into the red salt, colouring blue the resin of guaiacum, destroying all organic colouring matters, changing sulphuret of lead into a sulphate, &c.: the facts stated are sufficient to prove satisfactorily that ozone is not identical with hyponitric acids. I have, however, some other facts to mention which will demonstrate in the most direct manner, that ozone has nothing to do with the acid mentioned.

Ozone is abundantly produced (by means of phosphorus) in moist mixture of perfectly pure oxygen and hydrogen, or oxygen and carbonic acid. Ozone is generated by electrical sparks out of the purest oxygen obtainable, provided that principle contain some moisture. Ozone makes its appearance round the positive electrode, if a current passes through acidulated water containing no trace of nitrogen or any azotic compound. We are therefore allowed to consider it as a settled matter, that ozone is different both from nitrous and hyponitric acids.

* See Les Archives de l'Electricité, No. 17, p. 24–34.

Now, if a nitrate happens to be formed in treating, for instance, hydrate of lime with ozonized atmospheric air, it seems to follow that the nitric acid formed under those circumstances, owes, in some way or other, its origin to ozone, pure atmospheric air being unable to produce a nitrate with the hydrate named. But in what manner does ozone contribute to the formation of that acid? My experiments have shown that ozone is a very powerful oxidizing agent, so much so, that even iodine and silver are at the common temperature transformed into iodic acid and peroxide of silver. If it be a fact that the chemical powers of the oxygen contained in ozone are so much exalted as to enable that oxygen to unite with substances so little oxidable as iodine and silver are, it would not appear in my eyes very surprising if that same oxygen should happen to combine, under certain circumstances, with nitrogen to form what is called nitric acids.

According to my opinion (see my little work on the Slow and Rapid Combustion of Bodies) there does not exist a compound of nitrogen $=NO_5$. I consider what chemists call the first hydrate of nitric acid as $NO_4+HO_2$, and the normal nitrates as $NO_4+RO_2$. Nitrate of lime is therefore to me $NO_4+CaO_2$. Agreeable to that view, I am inclined to account for the nitrification above mentioned in the following manner. Ozone transforms first CaO into $CaO_2$, a change which, according to my recent experiments, a number of basic oxides undergo within an ozonized atmosphere. The oxides of manganese, lead, cobalt, nickel, and silver, for instance, are readily transformed by ozone into their corresponding peroxides. Whilst peroxide of calcium is formed, ozone exerts an oxidizing influence also upon the nitrogen contained in atmospheric air, and forms $NO_4$, which uniting to $CaO_2$, produces what we call nitrate of lime.

The fact, that electrical sparks passing through a moist mixture of oxygen and nitrogen generate nitric acid, was first ascertained by Cavendish, 1785; in 1840 I made the observation that under the same circumstances ozone is produced. Davy has found that traces of nitric acid are formed around the positive electrode if a current passes through water containing atmospheric air or nitrogen, and I have ascertained that under the same circumstances ozone is generated.

From the facts above stated, it appears that during the slow combustion which phosphorus undergoes in moist atmospheric air nitric acid is formed, and we are now well aware that ozone makes its appearance at the same time. The invariable concomitance of the two phenomena mentioned, *i. e.* the contemporary formation of nitric acid and ozone which takes place under circumstances apparently so very different from each other, gives room to suspect that the production of one of those compounds is in some way or other connected with the generation of the other.

As electrical sparks are able to produce ozone out of moist and otherwise pure oxygen, it appears that the formation of that substance is entirely independent of the generation of nitric acid, and it becomes highly probable that the ozone produced, under electrical influence, out of moist atmospheric air has nothing to do with the

nitrogen of the atmosphere. Now if ozone be capable of forming nitrate of lime by being treated both with nitrogen and hydrate of lime, it is very possible that ozone coming in the very moment of its formation in contact with nitrogen and water, produces what is called hydrate of nitric acid. Nitrogen being oxidized by ozone into $NO_4$, this compound would unite with an equivalent of $HO_2$ (ozone) and form hydrated nitric acids.

According to that view, the nitric acid generated,—

1. On electrifying moist atmospheric air;
2. On electrolysing water containing nitrogen, *i. e.* air;
3. On making phosphorus to act upon a moist mixture of oxygen and nitrogen;

would be formed in a secondary way, that is to say, in consequence of the oxidizing effects produced by ozone upon nitrogen.

Supposing the suspected connexion really exists between the formation of ozone and the generation of nitric acid, we can easily understand the concomitant appearance of both, and conceive that where ozone is produced, nitric acid also must be formed, provided there be nitrogen and water present at the spot where ozone happens to be generated. If, agreeably to the views hitherto developed, the nitric acid formed on electrifying moist atmospheric air; electrolysing water charged with some azote; or making phosphorus to act on a moist mixture of oxygen and nitrogen, owes its origin to the ozone being produced under those circumstances, what is the cause of the generation of that acid which is formed during the putrefaction of azotic substances being in contact with strong bases and air? The putrefaction of those substances consisting, as it is well known, partly at least, in an act of oxidation or slow combustion of their oxidable constituent parts, and it being a fact that during the slow combustion of phosphorus, vapour of ether, or alcohol in atmospheric air, a highly oxidizing agent (ozone) makes its appearance, we may be allowed to compare the slow combustion of phosphorus with the putrefaction of azotic organic matters, both of them taking place in the atmosphere. And if it be a fact that during the slow combustion of phosphorus and ether, ozone is produced, it does not seem to be an over-bold conjecture to suppose that ozone is also formed during the putrefaction of azotic substances, and the nitrogen of those matters oxidized into nitric acid. As phosphorus which contains no trace of nitrogen, nevertheless forms nitric acid on its being slowly burned in atmospheric air, it seems to be not impossible that organic substances containing no nitrogen produce also ozone, and along with it nitric acid during the slow oxidation they undergo in atmospheric air. It seems to me to be important to ascertain, by accurate experiments, whether, under the circumstances last mentioned, nitric acid or nitrates are formed. Some observations made by Braconnot and Saussure seem to speak in favour of such a formation, they are however too vague to decide the question. To arrive at certainty regarding that fact is important, not only with respect to the theory of spontaneous nitrification, but also with reference to a matter which has of late been the subject of much discussion.

From what source does the vegetable world draw the nitrogen which we meet in so many of her products? If, according to a prevailing notion, it be only nitrogen in a compound state that can be assimilated by plants, is it not possible that nitrogen contained in nitrates might be as well taken up by them as the azote of ammonia? It is easy to see that in the affirmative case some nitrogen would be offered to growing plants by the means of organic substances containing no azote, and putrefying in the atmosphere. I do not intend, however, to meddle with those transcendent questions of physiological chemistry, and leave the answering of them to those who are much better qualified than myself to solve problems of such an intricate and difficult nature.

*Bâle, October* 1845.

# XII. *Illustrations of the Viscous Theory of Glacier Motion.*

## Part I. *Containing Experiments on the Flow of Plastic Bodies, and Observations on the Phenomena of Lava Streams.*

*By* JAMES D. FORBES, *Esq., F.R.SS. L. and E., Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.*

Received March 15,—Read April 10, 1845.

§ 1. *Plastic Models.*
§ 2. *Analogy of Glaciers to Lava Streams.*
*Note on the Velocity of Lava.*

### § 1. *Plastic Models.*

IN the concluding chapter of my "Travels in the Alps of Savoy," I have shown how the obscure relations of the parts of a semifluid or viscous mass in motion (such as I have attempted to prove that the glaciers may be compared to) may be illustrated by experiment.

The larger models, these described and figured, showed very clearly the precise effects of friction upon the motion of such a mass. They were formed of plaster of Paris, mixed with glue, and run in irregular channels, and the relative velocities of the top and bottom, the sides and centre of such a pasty mass were displayed by the alternating layers of two coloured pastes, which were successively poured in at the head of the model valleys. The boundaries of the coloured pastes were squeezed by the mutual pressures into greatly elongated curves whose convexity was in the direction of motion; and in a vertical medial section, the retardation of the bottom and the mutual action of the posterior and anterior parts, shaped the bounding surface of two colours into a spoon-like form.

Now these models convey a very palpable commentary upon the effects of friction on a plastic mass, and likewise on the influence of the mutual pressures of its parts; but in further illustration of the same thing I constructed another model, only executed as the printing of my volume approached its close, and which is cursorily described in a long note (page 377)*, whence its real importance may perhaps have been pretty generally overlooked.

The models in question, of which I have since made many, are formed by accumulating in one end of a long narrow box, AB, Plate IV. fig. 1, a deep pool of the viscid

* In this paper reference is of course made to the first edition of my "Travels," the second not having been then published.

material already mentioned, which is retained there by a sluice or partition C which may be withdrawn at pleasure.

The surface of the pool *abcd* is then pretty thickly dusted over with a coloured powder, and the sluice is withdrawn.

The pasty mass subsides slowly under its own weight into the lengthened form *efgh*. The film of colour on the surface is therefore broken up so as to cover three or four times the surface it did at first; and its new distribution marks the lines of greatest separation of the superficial particles of the mass. The appearance of such a model when *run* is shown in fig. 2, and it manifests in the plainest manner the twofold tendency to separation in such a case where the channel is narrow and confined, and there is a certain mass of matter in front. Plate V. shows a more accurate drawing taken from such a model.

The lines of *sliding* separation occur most distinctly marked near the sides, where the friction is greatest, and the central parts are *forced past* the lateral parts, on account of the less embarrassed and consequently swifter motion of the centre; and they incline to the centre although the breadth of the channel be perfectly uniform. But the forces which tear asunder the parts (when such exists) act *perpendicularly to the former* and produce dislocations and fissures, which perfectly correspond to the direction and appearance of the crevasses of a glacier, that is, they are convex upwards or towards the origin of the glacier. It is the former of these lines of separation, or *differential motion*, which constitute and trace out with an exact parallelism the *veined structure* which I have described as forming the normal structure of all true glaciers. Plate V. is a representation of a very beautiful plaster model of more consistence than the other, in which the swelling of the surface and the direction of the open cracks produced by direct thrusts are most beautifully shown; and are even more so in the model than in the engraving. The fissures are transverse and slightly convex to the origin in the higher part of the glacier, then gradually turning round they radiate from a centre in the lower part, exactly as in the glacier of Arolla (Travels in the Alps, Plate VI.), and in all similar cases.

The experiment above detailed was suggested to me by studying the ripple of streams of water, which appear to have the same origin: and in very weak currents moving through very smooth and uniform channels (as the chiseled sides of water conduits) the same may be made manifest by throwing a handful of light powder on the surface, which then becomes divided into threads of particles inclined in the manner I have described at a certain angle from the side towards the centre, depending on the velocity of the stream.

The slightest prominence of any kind in the wall of such a conduit, a bit of wood or tuft of grass is sufficient to produce a well-marked ripple-streak, from the side towards the centre, depending upon the sudden and violent retardation of the lateral streamlets and the freer central ones being momentarily edged away from them. The general course of the motion of the particles is, however, scarcely affected by

such a circumstance, for the differential velocities which cause the ripple and the separation, are always small compared to the absolute velocity of the stream; and thus a floating body on the water (just as the moraine on the glacier) perseveres in its course parallel to the side with scarcely any perceptible disturbance. When however the descent is violent and the friction great, floating bodies are gradually drawn towards the centre, and this happens also in exactly the same circumstances to the moraine of the glacier. Plate IV. figs. 3 and 4, shows the relation of the ripple-marks to the channel of a very flat smooth gutter in one of the side streets of Pisa, sketched after heavy rain.

These ripple-marks in water are well seen near the piers of a bridge, or when a post is inserted in a stream and makes a fan-shaped mark in the water cleft by it: such marks have been much neglected by writers on hydraulics; but in one of the most ancient hydraulic treatises, that of Leonardo da Vinci, lately printed from the MS. in the Italian collection of writers on hydraulics, they are very well described and figured. A case parallel to the last-mentioned, where a fixed obstacle cleaves a descending stream and leaves its trace in the fan-shaped tail, is well seen in several glaciers, as in that at Ferpêcle, and the Glacier de Lys on the south side of Monte Rosa, particularly the last, where the veined structure follows the law just mentioned. And I desire here to record that the views just presented as to the origin of the veined structure of ice, were confirmed, but were not suggested, by the experiments on viscous fluids just mentioned. The necessity of the tearing up of a solid mass, if it moved at all in a bed presenting insurmountable resistances on all sides, in directions such as the veined structure presents, was foreseen by me whilst dwelling amongst the glaciers themselves, at a distance from books or the means of experiment. The following extract from my Third Letter to Professor Jameson, written in 1842 from the remote village of Zermatt, contains the substance of all that I have since developed and illustrated at greater length and in different ways rather to meet the difficulties of others, than to confirm what was plainly fixed in my own mind.

"The glacier struggles between a condition of fluidity and rigidity. It cannot obey the law of semi-fluid progression (maximum velocity at the centre, which is no hypothesis in the case of glaciers, but a fact), without a solution of continuity perpendicular to its sides. If two persons hold a sheet of paper so as to be tense, by the four corners, and one move two adjacent corners, whilst the other two remain at rest or move less fast, the tendency will be to tear the paper into shreds parallel to the motion; in the glacier the fissures thus formed are filled with percolated water, which is then frozen. It accords with this view,—1st, that the glacier moves fastest in the centre, and that the loop of the curve described coincides (by observation) with the line of swiftest motion. 2nd. That the bands are least distinct near the centre, for there the difference of velocity of two adjacent stripes parallel to the length of the glacier is nearly nothing; but near the sides, where the retardation is greatest, it is a maximum. 3rd. It accords with direct observation that the *differ-*

*ence* of velocity of the centre and sides is greater near the lower extremity of the glacier, and that the velocity is more nearly uniform in the higher part; this corresponds to the less elongated forms of the loops in the upper part of the glacier. 4th. In the highest part of such glaciers, as the curves become less bent the structure also vanishes. 5th. In the wide saucer-shaped glaciers which descend from mountain slopes, the velocity being as in shallow rivers nearly uniform across their breadth, no vertical structure is developed. On the other hand, the friction of the base determines an apparent stratification, parallel to the slope down which they fall. 6th. It also follows immediately (assuming it as a fact very probable, but still to be proved, that the deepest part of the glacier moves slower than the surface) that the *frontal dip* of the structural planes of all glaciers diminishes towards their inferior extremity, where it approaches zero, or even inclines outwards, since then the whole pressure of the semi-fluid mass is unsustained by any barrier, and the velocity varies (probably in a rapid progression) with the distance from the soil; whilst nearer the origin of the glacier the frontal dip is great, because the mass of the glacier forms a virtual barrier in advance, and the structure is comparatively indistinct, for the same reason that the transverse structure is indistinct, viz. that the neighbouring horizontal prisms of ice move with nearly a common velocity. 7th. Where two glaciers unite, it is a fact that the structure immediately becomes more developed. This arises from the increased velocity, as well as friction of each due to lateral compression. 8th. The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory; for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases; each mass moves singly, and the structure disappears very gradually*."

In explaining the theory of the veined structure at a meeting of the Royal Society of Edinburgh on the 20th of March 1843, I stated that I had arrived at the conclusion that crevasses resulting from tension in certain parts of a glacier, must be formed at right angles to the surfaces of discontinuity or structural veins where they intersect the surface: a law conformable to the empirical one discovered by me on the glacier of the Rhone in 1841†, since generalized in other cases, and which even the adversaries of my theoretical views have admitted to be a correct statement of the facts‡.

My attention was at that time (March 1843) turned by my learned and acute friend Mr. W. A. Cadell, to the veined structure of the slag of iron furnaces as due to the difference of velocity of the parts producing surfaces of separation and peculiar molecular condition. The transition was easy to the case of volcanic rocks and lava

* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842.

† Edinburgh Philosophical Journal, January 1842.

‡ Bibliothèque Universelle, tome xliv. p. 153. "C'est en effet un fait assez général que les bandes bleues coupent à angle droit les crevasses," &c.

streams, and this case was pressed on my attention by an unexpected journey which I soon after undertook to Italy and Sicily.

### § 2. *Analogy of Glaciers to Lava Streams.*

There is something pleasing to the imagination in the unexpected analogies presented by a torrent of fiery lava and the icy stream of a glacier. But when we look upon the comparison historically and critically, and find how generally this analogy has been perceived and adverted to by persons of very different views and talents of observation, we are strongly tempted to suspect that some latent cause confers the marked resemblance.

This cause I of course consider to be the laws and condition of their motion, the struggle of a semi-fluid mass of enormous weight creeping down a mountain side, in which fluidity and solidity are so curiously combined, that we should be at a loss in either case how to name it; a straining, crackling, splintering solid, heaved on by the internal energy of the latent fluidity which pervades it, and which at last succeeds in giving to the general character of the motion and the moving mass, those of fluid bodies subject to the law of gravity; whilst the parts, themselves almost rigid, have that rigidity most fantastically subjected to the action of the dominant principle.

In illustration of what has now been said, I shall quote passages from some authors which, without particular research, have come under my notice expressive of the analogy just mentioned.

Mrs. Starke, the author of a well-known guide book of Italy, published many years ago, speaks of having seen near the crater of Vesuvius in 1818, "five distinct streams of fire issuing from two mouths, and rolling wave after wave slowly down the mountain with the same noise (?) and in the same manner *as the melting glaciers roll into the valley of Chamouni*; indeed this awful and extraordinary scene would have brought to mind the Montanvert, had it not been for the crimson glare and excessive heat of the surrounding scoriæ*."

Mr. Auldjo, an intrepid alpine traveller, writing about Vesuvius, in 1832, says, "The field of lava in the interior of the crater, inclosed within a lofty and irregular bank, might be likened to a lake whose agitated waves had been suddenly petrified; and in many respects resembles the *Mers de Glace*, or level glaciers of Switzerland, although in its origin and materials so very different†." And the view in the same work of "streams of lava on the south-east of the cone" presents a perfect analogy to a glacier, bearing on its surface three medial and two lateral moraines.

Captain Basil Hall, writing of Vesuvius at a later period, uses these remarkable expressions whilst describing an eruption of lava:—"The colour of this stream was a brilliant pink, much brighter at the sides than in the middle, where either from the cooling of the surface, or the accumulation of cinders and broken pieces of stone, a

* Starke's Traveller's Guide, Ninth Edition, p. 293.

† Auldjo's Sketches of Vesuvius, p. 10, published 1833.

sort of dark ridge or backbone was visible from end to end, not unlike the moraine on the top of a glacier. This reminds me of a curious analogy which often struck me, between two objects so dissimilar as a glacier and a lava stream. They are both, more or less, frozen rivers; they both obey the law of gravitation with great reluctance, being essentially so sluggish, that although they both move along the bottoms of valleys with a force well nigh irresistible, their motion is sometimes scarcely perceptible*." This remarkable passage, worded with the usual scrupulous care of the author, combined with his account of the mechanism of a glacier in the description of the glacier of Miage in the same work, show that he had arrived at more correct notions on the subject than any of his contemporaries; notions which chiefly required careful observation to give them the force of demonstration. The allusion to *moraines* as characteristic of lava streams as well as glaciers, in the preceding extract, is perfectly borne out by the view of the lava of 1831 given by Mr. Auldjo and already cited; the same appearance is mentioned by M. Elie de Beaumont in his account of Etna in the following terms: "Une des circonstances que les coulées de lava présentent le plus invariablement * * * * consiste en ce que chaque coulée est flanquée de part et d'autre par une digue de scories accumulées qui rapellent par sa forme la moraine d'un glacier, * * * souvent aussi les coulées présentent de pareilles digues vers leur milieu, lorsqu'elles sont partagées en plusieurs courants distincts coulant l'un à coté de l'autre†."

In another place the same author compares the movement of the upper crust of the lava to that of glaciers according to the then prevalent theory:—"L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certaine épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de sa couche inférieure, se trouve contraint de glisser‡."

Finally, M. Rendu, Bishop of Annecy, in his excellent Essay on Glaciers, refers in one passage (and I believe in one only) to the possible analogy with a lava stream, "[le glacier] s'affaisse-t-il sur lui-même pour couler le long des pentes comme le ferait une lave à la fois ductile et liquide§?"

The following considerations seem to show more than a general external analogy between lava streams and glaciers.

Their velocities are sometimes equally slow. Although common lava is nearly as liquid as melted iron, when it issues from the orifice of the crater, its fluidity rapidly diminishes, and as it becomes more and more burdened by the consolidated slag through which it has to force its way, its velocity of motion diminishes in an almost

* Patchwork, by Captain Hall, vol. iii. p. 118, published 1841.

† Recherches sur le Mont Etna, p. 184. Published in the Mémoires pour servir à une Déscription Géologique de la France, tome iv. 1838.

‡ Ibid. p. 177.

§ Théorie des Glaciers, Mém. de l'Académie de Savoie, tome x. p. 93, published 1841.

inconceivable degree, and at length, when it ceases to present the slightest external trace of fluidity, its movement can only be ascertained by careful and repeated observations, just as in the case of a glacier. In November 1843, I watched lava issuing rapidly from a small mouth in the crater of Vesuvius at the rate of about one foot in a second. The eruption of Etna in 1832 advanced at the rate of five miles in two days, which is at the rate of one foot in about six seconds*. We may contrast with this the eruption of Etna in 1614, which yielded a lava which advanced but two miles in *ten* years according to Dolomieu †, during the whole of which time its motion was sensible. This gives a mean rate of rather more than three feet per day; but at the conclusion it was no doubt much slower.

Mr. Scrope ‡ saw the lava of 1819 in the Val del Bove moving down a considerable slope at the rate of a yard a day, nine months after its eruption. It had, he adds, the appearance of a huge heap of rough cinders; its progression was marked by a crackling noise due to friction and straining, and "on the whole was fitted to produce *any other idea than that of fluidity*. In fact," he continues, "we must represent to ourselves the mode in which the crystalline particles of lava move amongst one another, rather as a sliding or slipping of their plane surfaces over each other, facilitated by the intervention of the elastic (?) fluid, than as the rotatory movement which actuates the molecules of most other liquids." It is generally conformable to this view that we find in Hamilton's *Campi Phlegræi* (fol. 1. 38. *Note*) the curious remark that some lava is so incoherent, or whilst fluid has so little *viscosity*, that in issuing from the volcano (Vesuvius) it has appeared "*farinaceous*, the particles separating as they forced their way out, just like meal coming from under the grindstones."

From all this it is quite clear that the seeming rapidity of the parts of a glacier, or the slowness of its motion, cannot be taken as the slightest evidence of its moving otherwise than as a fluid, contending with the *rigor* of the parts which include and resist the moving force, which is truly hydrostatic though limited in its exercise.

It is manifestly futile and unphilosophical to seek one *cause* of motion in a lava which, like that of Vesuvius in 1805, must have described as many hundred feet in a *minute* as that of 1614 from Etna probably did in a *year* §; for the *mean* daily motion of the latter during *ten years* was three feet; but toward the end of that time it must evidently have had for a long period an average motion of one-half or one-quarter of this, and therefore below the observed mean movements of certain glaciers. Fluidity, in the first instance as in the second, was the propelling vehicle or manner in

* E. de Beaumont, Recherches sur le Mont Etna.

† Quoted by E. de Beaumont, p. 85. The original is in the Journal de Physique, vol. i. of the New Series, where it is mentioned that the same slowness of motion has been observed in lavas of Vesuvius. Ferrara (Descrizione del Etna. Palermo, 1818) denies this statement, but not I think on sufficient grounds.

‡ On Volcanoes, p. 102.

§ See Note on the Velocity of Lava Streams at the end of this paper.

which gravity acted, and this is a sufficient answer to any attempt to maintain that the plasticity of a glacier is a collateral but not a primary cause of motion,—a distinction surely without a difference.

As in the case of all imperfect fluids, the central and superficial particles move faster than the lateral and inferior ones; and when the fluidity is *exceedingly* imperfect, as in those long-flowing lavas, there must be a rupture of continuity between the parts to permit them to slide and jostle past one another. This is evidently the cause of the noise referred to by Mr. Scrope and other writers. This tearing up of the stream into longitudinal stripes, occasioned by the varying velocity of the parts, is thus described by M. Dufrenoy in his account of Vesuvius: "La plupart des coulées présentent des bandes longitudinales assez parallèles entre elles: ces larges stries saillantes sur la surface sont les traces du mouvement de la lave qui ne s'avance pas d'une seule pièce, mais par bandes parallèles*."

And M. Elie de Beaumont describes a lava stream at Etna in these terms: "La surface offrait de profondes cannelures parallèles entre elles, dirigées dans le sens du mouvement qui l'avoit déversée à l'extérieur et qui étaient croisées par *de nombreuses gerçures transversales*†." Here then is evidently the twofold system of rents and perpendicular fissures described in the commencement of this paper as being found in the models, and as being conformable to the phenomena of glaciers.

During the winter 1843–44 which I spent in Italy, I had an opportunity of testing these resemblances, and tracing others to glaciers in the lavas of Vesuvius and Etna. I entered on the inquiry with a very jealous care of being drawn into the admission of fanciful or imperfect analogies; and I shall confine myself to the statement of one or two most plain and undeniable confirmations, selected from the results of many fatiguing rambles.

The plastic nature of the viscous lavas of Vesuvius and Etna is such as well might obliterate any internal traces of rents due to differential velocity, which, in the mass, are speedily closed and reunited as in a stream of treacle, or in the plaster models before explained, where the interior is homogeneous and the superficial coating above is permanently dislocated.

In lavas the indescribable ruggedness of the surface very generally prevents any record of the gentler play of forces. The following facts appear to me quite conclusive as to the manner in which a mass partially solidified, yet moving as a fluid, is torn up by the interior forces which act upon it.

1. At Vesuvius, the *Fossa della Vetrana* between the Hermitage and Monte Somma, is a valley lined with the lava of 1751. I here observed that the lava was in some places detached from the wall of the valley, leaving a cavity on the sheltered side of a projecting elbow of rock, just as a glacier does in similar circumstances, showing the considerable consistence which the lava possessed.

* Dufrenoy sur les Environs des Naples, p. 324.

† E. de Beaumont, p. 38.

*In the upper part of this Fossa the lava has a distinct linear structure where broken, in shells parallel to the sides, whose thickness varies from one-third of an inch upwards.* The position of these surfaces of dislocation is indicated (for illustration) in figure 5 of Plate IV.

2. In the vast lava wastes of Etna, we encounter not only a greater extent of surface, but a greater variety of condition as to cohesion of the lava streams, and the slope down which it has descended, and thus we have a better chance of meeting with specimens of the manner in which the semi-solid crust of a lava stream is torn up and crevassed by the effect of gravity compelling it into the circumstances of fluid motion. From this tendency of all lavas to form slags, and of these slags to be splintered, tossed, and remoulded by the action of the still liquid portion of the stream below or around, not one-thousandth of the surface bears marks of the simple condition of fluidity under which it was originally moulded; and though when viewed from a distance, and in connexion with the form of the ground over which it has passed, we see plainly enough that it has *flowed* like a stream, the absence of any trace of easy undulating forms which characterise fluids or plastic masses, give to the *sciarre* of Etna (the *cheires* of Auvergne) an appearance far more removed from pristine fluidity than the glacier masses of Switzerland.

In traversing many miles of lava wastes between Nicolosi and Zafarana, on the eastern slope of Etna, I met with one singularly favourable specimen of a branch of a stream consolidated exactly as it had moved, and undisturbed afterwards. It is the part of the current of 1763, called *Lava delle Cerve*. The branch stream in question may be ten yards wide, and presents a thin crust, which has floated on the viscid lava below, and which, while yet imperfectly solidified, has been urged to move with the rest of the stream, and has undergone a process of division and rending accordingly. The stream has flowed in the direction from left to right in figure 6. The lateral parts PP, QQ have been *literally torn to pieces longitudinally* (as I wrote on my note-book on the spot) by the multiplied rents which showed the dislocation of the quicker moving central from the lateral parts, and these rents *inclined towards the centre of the stream in the direction in which it moved.* The length of the stream was divided by transverse rents strikingly convex towards the origin of the stream, as shown in the same figure. These cracks were marked by another peculiarity; the cake of floating scoria had not only been cracked across but pushed *upwards*, generally *forwards and upwards*, before it was finally included in the cooling mass of the stream; the result was the arrangement shown in the longitudinal section, fig. 7, which it will be seen resembles the tiling of a house, only that the fractured parts do not always overlap, but the anterior edge is tilted upwards. It will thus be seen that this tendency to separation acts also in the *vertical* plane, and the dotted lines *aa'*, *bb'*, &c. indicate the direction of its action, coinciding with the surfaces of differential motion, which produce what I have called the *frontal dip* of the veined structure of the ice of glaciers.

3. At no great distance from this lava, and near the foot of the hillock called the Serra Pizzuta, between the last-named point and the valley of Tripodo, I observed a *transverse* section of a lava stream exhibiting an arrangement in bands or plates, nearly parallel to the side of the current, but inclining towards the centre.

4. Between Zafarana and the Porta Calanna (Etna), a remarkably pretty illustration occurs in the surface of an old lava stream, worn and polished by the action of a brook. Where the lava has had to turn an abrupt corner of a rock, A, figure 8 (which represents a ground plan), the progress of the lava being violently checked by the resistance of the projecting mass, has been torn up into longitudinal shreds, which from imperfect fluidity have not reunited, but have left open cavities of the form represented in the figure, which exhibit with remarkable fidelity the forms of the fissures with which glaciers are sometimes traversed, when they are subjected to sudden transitions in their states of motion (as in the glacier des Bossons at Chamouni), and which coincide in direction with the veined structure, and pass into it by imperceptible gradations.

5. What I have called the *frontal dip* of the veined structure in glaciers*, I have explained by the accumulation of a sluggish mass of considerable extent upon a floor or bed offering the resistance of intense friction; in consequence of which the mass of ice, urged downwards and forwards by its intense weight, being resisted by the friction of that which immediately precedes it, must yield in the direction of least resistance, or squeeze itself in a slanting direction forwards and *upwards*, and thus sliding over the resisting mass immediately in front, will produce surfaces of discontinuity or differential velocity in that direction. Such a result I inferred from general principles without reference to any particular example, and the explanation of the superficial convexity of the lower part of many glaciers was evidently satisfactorily explained by it.

The convex swelling form of a viscous stream will depend principally upon the relative measure of two quantities, the stiffness or viscosity of the fluid, and the inclination of the surface; although it will also depend on the part of the stream, whether near the origin or the termination, which we consider.

I have found this variation from concave to convex, depending upon circumstances, alike in glaciers and lava streams. Some very highly inclined small glaciers existing at considerable heights, and therefore very hard and consistent, are, nevertheless, deeply concave from end to end, the slope compensating for the stiffness of the matter; such is a beautiful glacier, named, as far as I can learn, La Gria, or Glacier de Bourget, which descends from the Aiguille de Goutè towards the valley of Chamouni. See Plate IV. fig. 9.

Many, perhaps most, lava streams, where they have well-determined banks, are concave during the longer part of their course, but towards their termination they

* See my Travels in the Alps, 1st Edit. pp. 167, 376, and letter to Dr. Whewell in Jameson's Journal, Oct. 1844.

become convex as their viscosity increases. Nevertheless, I have seen portions of well-bounded streams decidedly convex.

The appearance of the termination of a lava stream approaches strikingly that of a glacier. But this is much more than a vague analogy, and the accounts of faithful eye-witnesses prove the resisted motion of the doughy stream to be such as I anticipated. We find it explicitly stated over and over again in the writings of Dolomieu* and Della Torre† (and more particularly by the latter), that when a lava stream meets with any obstacle in front which checks its course, or when its course is checked by its own sluggishness, the stream swells, and gains gradually in thickness by the fluid pressure from behind urging its particles *forwards* and *upwards*. So striking was this natural effect of semi-fluid pressure, that these old observers attributed it to a peculiar force developed in the lava, of the nature of "fermentation," producing intumescence, the only way by which they could account for the vertical rise of the fluid, although it was very evident that the result was only what might be expected from the nature of the lava. It was also observed that when the lava stream had thus attained a certain height, it began to move on again, the necessary result of the increased hydrostatic pressure, although attributed by the authors named to the heat developed by chemical action. The tenacity with which the idea was long adhered to, that the residual fluidity of a nearly cooled lava stream was insufficient to account for its progress, without attributing to it the qualities of a second volcanic focus, are curious proofs of how long a palpable cause may be rejected as insufficient to explain a phenomenon, and a totally imaginary one superadded‡.

I may add, that lava streams sometimes push their extremities *up hill*§; glaciers do the same.

In addition to the considerations already stated, which illustrate the viscous theory of glaciers, I am glad to avail myself of two which have reached me from independent and impartial sources.

The first is by Mr. Darwin, who in a small book on "Volcanic Islands," published about the time that I was engaged in making the preceding observations on Etna and Vesuvius, pointed out in a very clear manner the explanation which the veined structure of glaciers lends to that of volcanic rocks belonging to the Trachytic and Obsidian Series, where the lamination, instead of being obscure and rare, as it generally is in the Augitic lavas, owing perhaps to their greater fluidity, and more viscid and homogeneous texture, is the general rule. "The most probable explanation," says Mr. Darwin, "of the laminated structure of these felspathic rocks

* Papers in the Journal de Physique.

† Histoire du Vésuve. Naples, 1771, 8vo, p. 207–9, and several other places.

‡ See the view of the termination of a lava stream in Auldjo's Sketches of Vesuvius, facing p. 92. The reader may also compare the view of a grotto in the lava, in the same work, with that of the source of the Arveiron, in my Travels, p. 387.

§ Hamilton, Campi Phlegræi, folio, vol. i. p. 40, *note*.

appears to be that they have been stretched whilst flowing slowly onwards in a pasty condition, in precisely the same manner as Professor Forbes believes that the ice of moving glaciers is stretched and fissured. In both cases the zones may be compared to the finest agates; in both they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical*."

The other illustration is contained in a communication with which I have been favoured by Mr. Gordon, Professor of Civil Engineering in Glasgow, and which has been printed in the Philosophical Magazine for March 1845, to which therefore I may refer. I need only state at present that it demonstrates, from observations on the flow of Stockholm pitch with a speed wholly insensible, and which requires some months for its accomplishment even in small masses, that a motion, of the nature of fluid motion, takes place at temperatures at which the pitch remains so hard as to be fragile throughout, and presents angular fragments with a conchoidal fracture. Mr. Gordon adds, that the resistance of the pitch to its own forward motion produces bands of differential velocity and having the *frontal dip*.

*Edinburgh, February* 26, 1845.

*Note on the Velocity of Lava, referred to in p.* 149.

The following are a few facts which I have collected on the velocity of lava. That of Vesuvius in 1805 appears to be the most fluid on record. Von Buch, who was in company with MM. de Humboldt and Gay-Lussac, describes it as shooting suddenly before their eyes from top to bottom of the *cone* in one single instant†, which must correspond to a velocity of many hundred feet in a few seconds without interpreting it literally. Melograni, quoted by Breislak‡, says it described three miles in four minutes, or about seventy-five feet per second *at a mean*. The same lava, when it reached the level road at Torre del Greco, moved at the rate of only eighteen inches per minute, or three-tenths of an inch per second§. The lava of 1794 (Vesuvius) reached the sea, a distance of 12,961 feet, in six hours, or passed over one-third of a mile per hour, or eight inches per second‖; whilst the lava of Etna, in 1651, described sixteen miles in twenty-four hours, or above a foot per second the whole way. That of 1669 (Etna), which destroyed Catania, described the first thirteen miles of its course in twenty days, or at the rate of 162 feet per hour, but required twenty-three days for the last two miles, giving a velocity of twenty-two feet per hour¶; and we learn from Dolo-

* Darwin on Volcanic Islands, 1844. The whole passage, pp. 65–72, illustrates this analogy.

† Bibliothèque Britanique, vol. xxx. The vertical height of the cone proper is 700 or 800 feet; the length of the slope may therefore be 1300 feet.

‡ Institutions Géologiques, iii. 142.

§ Nicholson's Journal, vol. xii. ‖ Breislak, Campanie, i. 203.

¶ Ferrara, Descr. del Etna, p. 105. This appears from the dates, though at variance with one assertion of the author.

MIEU, that this same stream moved during part of its course at the rate of 1500 feet an hour, and in others took several days to cover a few yards*.

The lava of 1753 (Vesuvius), starting with a velocity of 2500 feet per hour, soon diminished to sixty feet†, as did that of 1754 to the same‡; and of 1766 to thirty feet per hour§. The lava of 1831 (Vesuvius) moved over 3600 feet in twenty-six hours, and finally advanced steadily at the rate of ten feet an hour‖. The lava of Etna of November 1843, is said to have moved over three paces per second at the distance of a mile from the crater.

The stream of 1761 (Vesuvius), before it stopped flowing, advanced but three yards a day¶; and that of 1766, which continued moving for about nine months, moved over but a small space in that time. Had the attention of authors been equally directed to the *slow* as to the rapid advancement of lava, there is no doubt that we should find many instances besides these recorded by DOLOMIEU and SCROPE, of continuous movements of three feet, and even one foot a day, or less.

* DOLOMIEU Isles Ponces, p. 286. Note.

† DELLA TORRE, Histoire, &c., p. 196.

‡ Ibid. p. 130.

§ HAMILTON, Campi Phlegræi, i. 19.

‖ AULDJO, Sketches of Vesuvius, p. 79, with a sketch of the front of the stream whilst advancing at this rate.

¶ DELLA TORRE, p. 182.

# XIII. *Illustrations of the Viscous Theory of Glacier Motion.*

## Part II. *An attempt to establish by observation the Plasticity of Glacier Ice.*

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§ 3. De Saussure's *Theory*.
§ 4. *Modifications of* De Saussure's *Theory*.
§ 5. *Experiments at Chamouni on the Plasticity of Ice.*

### § 3. De Saussure's *Theory*.

When Gruner proposed the explanation of glacier motion by the sliding of the ice over its bed, and De Saussure illustrated and confirmed it by considerations drawn from the lubricating action of the earth's heat melting the ice in contact with the soil*, there is no reason to suppose that either of them thought it necessary to take into account the varying form of the channel through which the glacier had to pass, and the consequently invincible barrier presented to the passage of a rigid cake of ice through a strait or narrow aperture when it occurred. This is the more remarkable, because he conceives that the *inequalities of the bed or bottom* may be overcome by the hydrostatic pressure of the water, which he supposes may be imprisoned between the rock and the ice, so as absolutely to heave the latter over the resisting obstacles.

I believe that in no part of De Saussure's writings will there be found any, the slightest reference to the possibility of the glacier when fairly formed *moulding* itself to the inequalities of the surfaces over which gravity urges it; nor is there any trace of the correlative fact of an unequal motion of the sides and centre of the ice, which may in some sense be considered as the geometrical statement of the preceding physical fact. The fact of plasticity was suspected by Basil Hall, and more distinctly announced by Rendu, as shown in the first part of this paper; but it could not be proved until the geometrical fact of the swifter motion of the centre of the glacier relatively to the sides was established in 1842†. The contrary opinion at that time

* To do Gruner justice, he appears to have been aware of the effects of the earth's heat and the lubricating action of the water thawed from the glacier: "Lorsque les côtés de l'amas [de glace] qui touchent la montagne, fondent en entier, toute la masse entraînée par son poids glisse sur son fond et s'avance dans la vallée," French translation, p. 333 ... "il est vraisemblable que leur surface inférieure [*i. e.* des glaciers] se liquéfie autant, et peut-être plus que la supérieure," ib. p. 289.

† Edinburgh Philosophical Journal, October 1842, and Travels in the Alps of Savoy, p. 134.

generally entertained would have been conclusive against the hypothesis of plasticity called forth by the gravity of the mass.

So far, then, as appears from his writings, De Saussure considered the ice of glaciers to constitute a mass possessing rigidity in the highest degree, such rigidity in short as common experience assigns to ice tranquilly frozen in small masses, which is sensibly inflexible. It is in this sense in which I have spoken of De Saussure's sliding theory, as one which "supposes the mass of the glacier to be a rigid one sliding over its trough or bed in the manner of solid bodies*," and I adhere to the definition as excluding the introduction of the smallest flexibility or plasticity, to which the term rigidity is correctly opposed. I consider too that De Saussure's theory supposes the mass of the glacier to slide over its trough or bed in the manner of *solid* bodies, that is, not as a heap of rubbish or absolute fragments, such as a glacier sometimes precipitates over a rock, but which evidently did not enter into De Saussure's explanation, nor, in fact, required any theory.

As to the crevasses which form so prominent a feature of many glaciers (although many are in parts almost devoid of them), I do not recollect that De Saussure alludes to them as *facilitating* in any way the movement of the glacier, but simply as *results* of its motion and of the rigid character of ice. And I believe that this view (whether it was held by De Saussure or not) is substantially correct. The regular system of crevasses of a glacier is approximately transverse, rather arched upwards towards the origin of the glacier, and as De Saussure supposes the glacier to be pressed downwards by the mass of snow accumulating at its head, it is hard to believe that he could have regarded these fissures as in any way essential to its movement, even were they very numerous; the tendency of such a pressure from above would rather seem to be to pack the ice like an arch, opposing its convex side to the direction of the pressure.

The view now given of De Saussure's theory of glacier motion is not only conformable to what may be gathered from his writings, but expresses the unanimous understanding of his numerous commentators, followers and opponents. As some doubt has lately been hinted as to the definiteness of De Saussure's conception of a glacier as a mass devoid of flexibility and plasticity and urged down a slope *as a whole*, by the lubricating action of fusion in contact with the soil to an extent which, in extreme cases, might even give it the character of buoyancy, I will take the liberty of quoting some indisputable authorities amongst writers of name in different countries.

And first from De Saussure himself:—

"La chaleur de la terre fait fondre les neiges et les glaces, même pendant les froids les plus rigoureux lorsque leur épaisseur est assez grande pour préserver du froid exterieur les fonds sur lesquels elles réposent."—*Voyages*, § 532. *** "C'est elle qui entretient les torrents, qui, même pendant les plus grands froids, ne discontinuent jamais de sortir de tous les grands glaciers." § 533. ***

* Travels, p. 362.

"Presque tous les glaciers reposent sur des fonds inclinés; et tous ceux d'une grandeur un peu considérable ont au-dessous d'eux, même en hiver, des courans d'eau qui coulent entre la glace et le fond qui la porte. On comprend donc que ces masses glacées, entraînée par la pente du fond sur lequel elles reposent, dégagées par les eaux de la liaison qu'elles pourraient contracter avec ce même fond, soulevées même quelquefois par ces eaux, doivent peu à peu glisser et déscendre en suivant la pente des vallées, ou des croupes qu'elles couvrent." § 535.

"Quand on considère que ces glaces reposent sur des plans inclinés, qu'il coule sous elles des torrens d'eaux qui les fondent par en bas, les détachent et les soulevent, ne sent-on pas que leur permanence dans la même place est une chose *physiquement impossible*?" § 2284.

RAMOND'S account:

"La cause [de la marche des glaciers] est facile à concevoir: une masse qui pèse sur un plan incliné tend nécessairement à déscendre, et cette tendance est favorisée dans les glaciers par le choc des torrents qui roulent sous leur voûtes, par l'humidité que leur masse communique au terrain qui les porte, enfin par cette multitude innombrable de cavités qui creusent leur partie inférieure, et dont l'effet est de diminuer le frottement en diminuant l'étendue des surfaces*."

ELIE DE BEAUMONT'S account of DE SAUSSURE'S theory: speaking of the lava of Etna, he says, "L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certain épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de la couche inférieure se trouve contraint à glisser†."

BISCHOFF'S account:

"Das jährliche Vorrücken der Gletscher welches SAUSSURE ganz einfach aus einem allmäligen Herunterrutschen der unteren durch das Aufthauen des Eises schlüpfrig gewordenen Seite des Gletschers auf der schiefen Fläche des Bodens erklärt, ist eine bekannte Thatsache‡."

AGASSIZ'S account:

"Autrefois on admettait tout simplement qu'ils glissaient sur leur fond, en vertu de leur propre pesanteur, et que ce glissement était favorisé par les eaux au fond de leur lit. C'était l'opinion de SAUSSURE§."

MARTINS' account:

"DE SAUSSURE, ESCHER DE LA LINTH, ANDRÉ DE LUC, attribuent cette progression au poids des glaces et à l'affaissement produit par la fonte de la face inférieure qui repose sur le sol‖."

* Voyages en Suisse par COXE, traduit par RAMOND, ii. 119, 1790.

† Mémoires pour servir à la Déscription Géologique de la France, iv. 177, published 1838.

‡ Wärmelehre, p. 180, published 1837. § Etudes sur les Glaciers, p. 152, published 1840.

‖ Sur les Glaciers de Spitzberg. Bibliothèque Universelle, 1840, tom. xxviii. p. 166.

STUDER's account:

"Die bisher fast allgemein herrschende Theorie erklärte die Bewegung der Gletscher aus der Schwere allein. Es soll die Gletschermasse als STARRER KÖRPER auf ihrer Felsgrundlage, wie auf einer schiefen Ebene, theils durch ihr eigenes Gewicht theils durch dem Druck der höheren Eis- und Firnmasse herunter gleiten (GRUNER, RAMOND, KUHN, DE SAUSSURE, ESCHER)*."

This last testimony of the most exact and most learned of the living Swiss geologists as to the sense in which DE SAUSSURE's theory has always been understood is so important that I shall add a translation: "The hitherto generally prevailing theory explains the movement of the glaciers by gravity alone. The glacier masses are considered as RIGID BODIES, which slide down over their rocky beds partly by their own weight, partly under the pressure of the higher ice and *névé*." My interpretation of the views of DE SAUSSURE as regards the rigidity of the glacier ice is thus borne out by an independent authority, for M. STUDER's work and my own appeared simultaneously. It is further confirmed by private communication with another eminent Swiss naturalist nearly connected by relationship with DE SAUSSURE himself, who is more intimately acquainted with the opinions and writings of his illustrious kinsman than any other person now alive, and who considers that DE SAUSSURE's views were confined to the general analogy of the glaciers to solid masses sliding down inclined planes, and that the effects of the inequalities of the channels and forms of the ice-basins were not comprehended in his theory.

If we feel surprise that a naturalist and observer so eminent had not adverted to the difficulty of imagining a solid cake of ice, even though perfectly detached from its bed, to disengage itself from the obstacles and sinuosities of its rocky channel, we should remember,—*first*, that the explanation is given in the most general terms, and there is no appearance that its author looked more closely at its consequences and details than to satisfy himself that a sliding motion in the abstract was rendered possible by the action of the earth's proper heat, an ingenious and philosophical element of the theory (however inadequate), and that which being due principally to DE SAUSSURE, renders the theory properly his, and connected it with his ingenious inquiry into this curious part of physics as a distinct and wholly independent investigation. *Secondly*. Every one knows how an application of a principle so true and so ingenious leads men of even the most exact habits of thought to overlook difficulties in a subject almost unstudied. DE SAUSSURE did much for our knowledge of glaciers, and he saw much which no one had observed before him: we must not blame him if, yielding to a true and natural analogy of sliding bodies, he overlooked real and great difficulties inherent in the conception of a glacier as a solid continuous mass and highly rigid. *Thirdly*. In DE SAUSSURE's time no plan or map, worthy of the name, of any glacier existed, and this was a blank which even DE SAUSSURE did not attempt to supply. The popular notion of a glacier, which it is certain he had in his mind

* Lehrbuch der physikalischen Geographie und Geologie, 1844.

when he penned the passages which relate to their motion, is a mass of ice of small depth and considerable but uniform breadth sliding down a uniform valley, or pouring from a narrow valley into a wider one, as is the case with a vast majority of glaciers tolerably accessible, and which alone were visited at the time of publication of the first edition of the *Voyages dans les Alpes*. In all these cases the lateral resistance might easily be overlooked, and the popular comparison to one solid body sliding on another and lubricated by its own liquefaction might be accepted as a complete explanation; as has even been done at a later period by those who have attempted to illustrate DE SAUSSURE's theory by experiment, but who, like him, neglected the form and undulations of the bed in which it rests.

### § 4. *Modifications of* DE SAUSSURE's *Theory*.

DE SAUSSURE and his immediate followers appear to have considered the crevasses which occur transversely in most glaciers, as the result of the inequalities of the beds down which they are constrained to move; but other writers have imagined that the part which these crevasses perform in the phenomena of glacier motion is fundamental, and essential to the existence of the movement at all. Some writers have remarked that the fall of ice blocks over the precipice which often occurs near the lower end of glaciers, leaving the superior portions unsupported, allows them to advance to fill the position formerly occupied by the portion of the now fallen ice. But in this case it would appear that cause and effect are in some degree confounded. The ice about to be projected over the cliff must either advance towards its fall by its own gravity, or by the pressure of the parts behind. If its own gravity suffices, the same cause will urge the ice behind it to move similarly, whether the block in question fall or not; and if it be the pressure from behind which shoves it on, then still more is the pressure of the entire glacier the cause of motion of the entire glacier, irrespective of the precipitation of its more advanced part.

Thus, M. MARTINS' theory of the progression of glaciers is, that the weight of the parts causes them to separate by fissures into wedge-shaped masses, without their sliding along the bottom; that the fissures become filled with frozen snow, and that thus the glacier is perpetuated and extended year by year. "Cette progression," he says, "n'est donc ni un glissement ni un affaissement difficiles à comprendre, puisque la glace doit adhérer au sol, mais un démembrement successif*." Besides other objections, it is now universally admitted that the glacier-proper does not grow by the consolidation of snow in its fissures.

But setting aside the attempt to render the sliding motion of the entire glacier considered as a plane slab more easy, by considering the motions of the parts instead of the motion of the whole, we are led to notice the attempt to reconcile the sliding theory to recent observation, by ascribing to the crevasses of the glacier the important office of enabling it to accommodate itself to the inequalities of its channel.

* MARTINS sur les Glaciers de Spitzberg et de la Suisse. Bibl. Univ. Juillet 1840.

Our object is, in this section, merely to state the view in its most plausible form, which in the succeeding section we shall controvert by experiments giving it a direct negative. In the third portion of this essay we shall enter more at large into the phenomena of crevasses, and mention other objections to this hypothesis and every modification of it.

According to this view, the friction of the ice against the sides of the valleys will produce a dislocation of the glacier into longitudinal stripes (as shown in Plate VII. fig. 1.*), where a transverse line $bb'$ becomes by the irregular motions of the ice distorted into the zigzag form $hcc'k$. Or if we suppose the plasticity of the ice to be sensible, but that its action is accompanied with fractures, the abruptness of the angles of the figure will be softened, as in the broken line $lmm'l$ in the lower part of the same figure. This latter hypothesis evidently merges into the true plastic theory, when the part of the progression due to the flexure of the transverse lines bears a large proportion to the effect of the longitudinal slide, or more generally, when the surfaces of sliding or yielding become greatly multiplied, when the notched line will merge into a curve.

The passage of the glacier through a gorge or contraction is explained on the same view by figure 2, where the resistance of the sides having occasioned a series of parallel longitudinal rents as before, the portion of the glacier beyond the limits of breadth of the gorge BB′ is supposed to be detained or embayed whilst the intermediate columns slip through.

### § 5. *Experiments at Chamouni on the Plasticity of Ice.*

It has been shown that in order to reconcile De Saussure's theory of sliding motion with the ascertained fact that the centre of the glacier moves faster than the sides, it had been assumed that solutions of continuity or longitudinal crevasses were formed parallel to the length of the glacier, by means of which the central portion slides past that adjacent to it, and so on for successive strips as we approach the sides, the more rapid retardation near the sides being rendered mechanically possible by the increased number of these longitudinal dislocations.

The result was therefore predicted to be that the glacier would be found to move by *echelons*, or that strips of ice of a certain number of feet, or yards, or fathoms, would move either suddenly or by gradual sliding, but at all events so as to mark by an abrupt separation at the longitudinal fissure, that the one portion of ice has slipped past the other by a distinct measurable quantity.

When I first learnt at Geneva, in August 1844, from Mr. Hopkins's published papers†, that this was really the author's meaning, it occurred to me that the proof

* These figures and their interpretation are taken from Mr. Hopkins's First Memoir in the Cambridge Transactions, vol. viii. part 1. A figure similar to the first is to be found in a more recent paper by the same author in the Philosophical Magazine for June 1845.

† Cambridge Transactions, vol. viii.

between the rival theories was easy, and that it was only necessary to place a series of marks in a right line transversely to the glacier, and observe whether they were displaced by an imperceptible flexure, or whether they slid past one another by sudden dislocations.

Such a proof was independent of any assertion as to the existence or not of such fissures as those contended for, about which different opinions might be formed, especially as they might be asserted to exist although invisible to the eye. Being satisfied in my own mind of the non-existence of such fissures wherever the ice is not violently dislocated and descends a steep place in a tumultuous manner (which, as already mentioned, is not the case which we consider), I had no hesitation in predicting that the result of the experiment would be confirmatory of my theory, and contradictory of the other; that the transverse line would be found to become a continuous curve, and that no other system of fissures could be found in the glacier satisfying the mechanical postulate of the greater velocity of the central parts of the glacier, than the *ribboned structure* of the ice, which I had already pointed out as resulting from a forced separation of the semi-rigid ice, at a vast, though finite, number of points in the breadth of the glacier, and which I showed to exist exactly in the direction required for releasing the mass from the tension induced by the gravity of its parts.

Having gone to Chamouni a few days later, I looked out for a place where the ice should be as compact as possible, wholly devoid of open fissures, and if possible continuous up to the bank. This latter condition I found it impossible to fulfil on the Mer de Glace, at least without ascending to the *névé*, which might be objected to as less rigid than the glacier proper. The former condition was well-satisfied in a sort of bay on the west side of the Mer de Glace between the Angle and Trelaporte, exactly under the little glacier of Charmoz. The part adjoining the western shore of the glacier is indeed highly crevassed, and therefore unfit for this experiment; but at the distance of fifty or sixty yards from the moraine it becomes remarkably flat and compact for a space of about seventy yards in width, and several hundred yards in length, throughout which space there is not a single open crevasse. Now this compact area of ice presents the veined structure in a nearly longitudinal direction, with a degree of delicacy and distinctness not to be found in any other part of this glacier (as I had already remarked in my *Travels*, p. 159), and it contains no other trace of a system of longitudinal fissures or lines of separation of any kind, which could render mechanically possible the distortion of this flat compact surface of so great an extent. Now I have always observed that the veined structure near the side of a glacier is best developed where the ice is least crevassed, or the continuity of the mass most perfect; a fact stated and referred to its true cause from the first date of my speculations on the origin of the blue veins, in the following words:—"The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory;

for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually*." Now the ice at the point in question is the compacted ice which has just passed round the great promontory of Trelaporte, having been rent by numberless chasms, and which is consolidated by pressure in the bay in question, whilst the centre of the glacier being still on the steep is deeply crevassed. The structure of the even ice is continuously striped with a regularity comparable to that of the finest chalcedony for a distance of some hundred feet. This structure must *have been produced on the spot*, since no such perfect structure exists higher up, and if it did, it must have retained all the marks of dislocation due to the formation and reconsolidation of the fissures, which are so numerous and wide as to render the passage of the glacier quite impracticable if we follow the same strip of ice up towards the promontory of Trelaporte. Let it then be recollected that the structure is *produced here*, under our eyes, on the very spot where the experiments about to be detailed were made, and that the structure in question produced a vertical slaty cleavage so distinct, that the ice broken into hand specimens may be split parallel to it like any slaty rock, and that the fine hard laminæ projecting vertically after the glacier has been washed by rain, permitted the blade of a knife to be thrust between them to a depth of several inches, although they are rarely more than a quarter of an inch thick.

I shall now describe the actual measurements made upon the glacier in order that my method of proceeding in similar cases (when I have only published results) may be understood.

The general position of the experimental surface will be understood from the topographical sketch (Plate VIII. fig. 1.) The theodolite was planted at a fixed point on the ice Q, just within the crevassed portion, which intervened between it and the western shore of the glacier. This point of fundamental and constant reference was fixed by an exactly vertical hole pierced with an iron *jumper*, or blasting iron, one inch in diameter, and was frequently deepened in order to preserve the centre as exactly as possible in the same vertical line in the ice. The theodolite was centred over it at every observation by means of a plummet, which nearly filled the cylindrical hole and permitted an adjustment, which one day with another might be accurate to about one-tenth of an inch. No stick was placed in the hole, but when not in use it was covered by a large flat stone, which effectually prevents congelation in ordinary weather†. The adjustment of the theodolite on the ice is always a matter for patience, but I succeeded in rendering it perfectly stable when once erected by inserting the three feet in cavities in the ice, and filling them carefully with ice chips.

* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842, and Appendix to Travels, 1st edit. p. 407.

† On one occasion this precaution having been neglected (in the case of a different mark on the ice), the hole was found completely frozen up after exposure to a day or two of severe weather in the month of August. It was however recovered by observing the beautiful stellar form of the ice-crystallization.

The theodolite, placed at Q, was pointed with its vertical wire on the well-defined angle of an erratic block Q1 on the opposite eastern bank of the glacier, above Les Echellets (see Plate VIII. fig. 2.). By causing the telescope to traverse in a vertical circle, a transverse line joining the points Q, Q1 was determined, and several stations were fixed in the compact ice eastwards of Q, at distances from it of 30, 60, and 90 English feet and subsequently at 120 and 180 feet. These were numbered in succession (1), (2), (3), (4), (5), and the permanence of their positions in the ice was secured as before by carefully driving vertical holes two feet deep, which were occasionally deepened, and covered with flat stones when not in use. As these points were in succession nearer to the centre of the glacier, they were expected to move with gradually increasing velocity in advance of the imaginary line Q, Q1 drawn across the ice.

But as the theodolite stationed on the glacier at Q must partake of its motion whilst the mark Q1 on the bank remained at rest, the visual line QQ1 would appear to revolve *towards* the origin of the glacier, and hence the relative advance of the points (1), (2), &c. would seem too rapid. To estimate the correction for this error the velocity of the glacier at Q must be determined, and also the distance QQ1. For the former purpose the following method was adopted. When an observation at station Q had been completed, by pointing the telescope on Q1 and observing the apparent advance of the points (1), (2), &c., the telescope was reversed in the Y's, or turned 180° towards the western moraine, upon which it indicated from day to day a new position, owing to the angular revolution of the line joining the fixed point Q1 and the moveable point Q. The point Q2 in the topographical sketch (Plate VIII. fig. 1) indicates the point where the visual line touched the moraine at the commencement of the observations on the 9th of August 1844. By the application of a scale or a similar method, the apparent advance of Q referred to the moraine Q2 was regularly measured. It is thus obvious that these apparent motions were *too great* (by the property of diverging lines) in the ratio of the distance Q1.....Q2, to Q1.....Q: and hence it became necessary to ascertain the position of Q2 as well as Q1. For this purpose a base-line of 300 feet was measured on the ice parallel to the length of the glacier, or perpendicular to the transverse visual line, extending from the point marked (3) to the point Q3 in fig. 3, whence by the theodolite the following bearings were taken:—

| | |
|---|---|
| Q1 . . . . . | 0° 0′ |
| (3) . . . . . | 83 6 |
| Q2 . . . . . | 148 0 |

From which we deduce the distance from

| | |
|---|---|
| Q1 to (3) . . . | 2479 feet. |
| (3) to Q2 . . . | 640 feet. |

But as (3) is 90 feet east of Q, we have

Q.....Q1=2569 feet. Q.....Q2=550 feet.

The apparent motion of Q measured on the moraine is greater than the true motion in the ratio $\frac{2569+550}{2569}$ or $\frac{6}{5}$ nearly. The actual motion of Q is readily deduced as well as the apparent rotation of the visual line QQ1. Thus during 16·75 days, the duration of the experiment, the apparent advance of Q referred to the moraine Q2 was twenty-three feet six inches, which at the distance Q1.....Q2 (3119 feet) subtends an angle of 25′ 54″, or almost exactly *one and a half minutes daily*.

The motion of Q during the interval of any two observations of the marks (1), (2), (3), &c. being thus known, the correction applicable to the apparent advancement of the said marks beyond the visual line is at once found by the proportion

Q.....Q1 : Q's motion :: Q.....(1) : E,

where E is the error of apparent position of mark (1). Thus, suppose the apparent motion on the moraine Q2 to be seventeen inches; this, reduced in the ratio of 6 : 5, gives 14·2 inches for the progress of Q. If the effect on the apparent place of a mark ninety feet from Q were required, we should have 2569 : 14·2 :: 90 : 0·50 inch.

I shall first detail the observations on the total motion of the glacier at Q, during the period to which the experiment extends, with the corrected daily motion and a memorandum of the state of the intervening weather, which accounts by its excessive variability for the remarkable variation of the progress of the glacier*.

TABLE I.

| Date. | Interval. | *Apparent* motion from commencement. | *Corrected* daily rate. | Weather. |
|---|---|---|---|---|
| | days. | ft. inch. | inch. | |
| August 9. 6 P.M. | 0 | 0 0 | | 9. Fine. |
| 10. 3½ P.M. | 0·90 | 1 5 | 15·6 | 10. Some rain. 11. Some rain. |
| 12. 2 P.M. | 1·94 | 4 0 | 13·2 | 12. Some rain. 13. Snow storm. 14. Some rain. 15. Snow storm. 16. Wet. |
| 17. 2½ P.M. | 5·02 | 11 6½ | 14·8 | 17. Fine; melting snow on glacier. 18. Showery. 19. Fine; snow still on glacier. 20. Very fine. 21. Very fine. |
| 20. 1 P.M. | 2·94 | 16 0 | 15·0 | 22. Rain. |
| 23. 2 P.M. | 3·04 | 19 10 | 12·5 | 23. Showery. 24. Cold rain. 25. Fine. |
| 26. Noon. | 2·92 | 23 6 | 11·8 | 26. Fine. Glacier dry. |

The first three marks on the ice, those placed thirty, sixty, and ninety feet nearer the centre of the glacier than Q, were fixed on the 9th of August, the mark (4) at 120 feet was planted on the 17th, and the mark (5) at 180 feet, on the 19th. The following are the observations on the *apparent* motions of these points past the transversal line through Q, as well as these relative motions corrected for the real movement of the station Q, as explained in last page. To avoid an illusory appearance of accuracy, the results are given to the nearest twentieths of an inch, which is below the possible errors of observation.

* See Travels in the Alps of Savoy, &c., p. 148.

## Table II.

Table of the *Apparent* and *True* Motions of the Stations *relatively to Station Q.*

| Date. | Interval. | Motion of Q. | Apparent relative motions. | | | | | True relative motions. | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | 30 feet. | 60 feet. | 90 feet. | 120 feet. | 180 feet. | 30 feet. | 60 feet. | 90 feet. | 120 feet. | 180 feet. |
| | days. | inch. | inch. | inch. | inch. | inch. | inch. | inch. | inch. | inch. | inch. | inch. |
| August 9. 6 P.M. ..... | 0·0 | 0·0 | 0·0 | 0·0 | 0·0 | | | 0·0 | 0·0 | 0·0 | | |
| 10. 3½ P.M. ..... | 0·90 | 14·0 | 0·8 | 1·9 | 2·3 | | | 0·65 | 1·6 | 1·8 | | |
| 12. 2 P.M. ..... | 1·94 | 25·6 | 1·7 | 3·1 | 3·7 | | | 1·4 | 2·5 | 2·8 | | |
| 14. 1 P.M. ..... | 1·96 | 29·0 | 1·5 | 2·8 | 4·3 | 0·0 | | 1·15 | 2·1 | 3·25 | 0·0 | |
| 17. Noon..... | 2·96 | 44 | 2·1 | 4·0 | 6·0 | 7·1 | 0·0 | 1·6 | 2·95 | 4·45 | 5·0 | 0·0 |
| 19. 5 P.M. ..... | 2·21 | 33·2 | 1·7 | 2·7 | 4·4 | 6·2 | 8·5 | 1·4 | 2·1 | 3·25 | 4·75 | 6·15 |
| 23. 1 P.M. ..... | 3·83 | 48 | 2·5 | 5·8 | 7·7 | 10·3 | 15·0 | 1·95 | 4·7 | 6·05 | 8·15 | 11·75 |
| 26. Noon..... | 2·96 | 34·6 | 2·1 | 4·6 | 6·5 | 8·0 | 11·0 | 1·7 | 3·6 | 5·3 | 6·4 | 8·6 |
| Motion from 9th to 26th... | 16·76 | 228·4* | | | | | | 9·85 | 19·55 | 26·9 | | |
| Motion from 17th to 26th... | 11·96 | 159·8 | | | | | | 6·65 | 13·35 | 19·05 | 24·3 | |
| Motion from 19th to 26th... | 9·00 | 115·8 | | | | | | 5·05 | 10·4 | 14·6 | 19·3 | 26·5 |

The results in the preceding Table have been divided into three periods, corresponding to the unequal times of observation of the last two and the first three stations. The first line of addition includes the motion at thirty, sixty, and ninety feet for nearly seventeen days; the second sum contains the comparative results for four stations throughout twelve days, and the last line contains the entire relative motions of five stations for nine days. These results may be further analysed, as in the following Table, which exhibits the mean daily motion in inches corresponding to each point for these distinct periods, and also the ratio of the relative motion of each point to the actual motion of the glacier at Q, or the zero point, during the same interval.

## Table III.

| Interval in days. | Actual motion of Q, or zero point. | (1.) 30 feet. | | (2.) 60 feet. | | (3.) 90 feet. | | (4.) 120 feet. | | (5.) 180 feet. | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Mean daily relative motion. | Ratio to actual motion. | Daily relative motion. | Ratio to actual motion. | Daily relative motion. | Ratio to actual motion. | Daily relative motion. | Ratio to actual motion. | Daily relative motion. | Ratio to actual motion. |
| | inch. | inch. | | inch. | | inch. | | inch. | | inch. | |
| 16·76 | 228·4 | 0·59 | 0·043 | 1·17 | 0·086 | 1·60 | 0·118 | | | | |
| 11·96 | 159·8 | 0·56 | 0·042 | 1·12 | 0·084 | 1·59 | 0·119 | 2·03 | 0·152 | | |
| 9·00 | 115·8 | 0·56 | 0·044 | 1·16 | 0·090 | 1·62 | 0·126 | 2·16 | 0·167 | 2·94 | 0·229 |

This Table shows, first, in a striking point of view, the regularity of action of the law by which the variable motion of the different transversal points in the glacier is governed, since the movement in the different intervals bears so near a proportion, that when estimated in terms of the actual motion of the glacier at the place, the relative motion of the parts scarcely differs by unity in the second place of decimals, and is generally much under it. Taking into account the inevitable errors of observation and the extraordinarily unfavourable circumstances of the weather, it is in the very highest degree improbable that this *law of continuity* of the *partial* motions can be ac-

* The true sum ought to be about four inches greater. The difference arises from the impossibility of estimating the correct velocities for the fractional intervals.

counted for by any casual justling or sliding of one finite portion of the ice past another, which would inevitably have left some of the points relatively at rest during some one of the many intervals of observation, and given to others evidence of a starting motion until friction had established a fresh position of repose amongst the struggling masses.

Secondly. This Table enables us to establish not only the continuity of motion of any one point, but the continuity of the relation which connects the points (1), (2), (3), &c. For instance, the relative motions of (1) being

·59 ·56 ·56,

and those of (2) being

1·17 1·12 1·16,

the ratios are

1·98 2·00 2·07.

In like manner the ratios between (3) and (2) will be found to be

1·37 1·42 1·44.

Thirdly. The flexure of the ice may be conveniently represented by a diagram, in which the several ordinates are set off corresponding to the relative spaces moved over. But to find the initial positions of the fourth and fifth marks, the proportional motion for the first period, when they were not observed, must be deduced from the comparative velocity of the period when the observations were comparable. Thus by Table II. the relative velocity of (3) to (4) during the time that they were observed together is 19·05 : 24·3; consequently whilst (3) moved over 26·9 inches (4) would have moved over 34·3 inches; the proportional motion for 16·75 days. In like manner for the mark (5) we have the simultaneous motions of (3) and (5) expressed by 14·6 inches and 26·5 inches, and hence by proportion, as before, we find

14·6 : 26·5 = 26·9 : 48·8 inches,

the relative motion of (5) in 16·75 days.

From these data the simultaneous relative motions of these six stations may be projected in a curve, or rather polygon, as shown in Plate IX. fig. 1. This is interesting, as showing very plainly, not only the regulated increase of swiftness of the glacier towards the centre, but that the *variation of the variation* is clearly brought out, indicated by a convexity in the direction of the motion, and confirming the general principle long ago announced by me, that the retardation is relatively greatest towards the side and less towards the centre. I appeal to any one conversant with the laws of mechanics in their practical application, whether the manifest continuity of such a law does not plainly include a continuity in the mutual action of the parts of the mass under experiment, and even independent of the manifest absence of great dislocations, would not establish the doctrine of a molecular yielding, or plasticity in the ice as opposed to the irregular justling of great blocks, admitting that such could exist unperceived.

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The period through which this experiment extended (seventeen days) is conclusive against the idea that a small flexure could take place until the accumulated strain on the solid produced a rupture, which relieved the strain, and so forth, *per saltus*. The continuity of glacier motion in every case except that of precipitous descents or ice-falls, first proved by my experiments in 1842, is now universally admitted by those who have had any personal experience in the measurement of glacier motion, however, opposed to my theoretical views*. The changes for seventeen days were connected (as has been shown) by a law of continuity established by numerous intervening observations; and the flexure or distortion of the ice amounted in this time to no less than *four feet* at the opposite ends of a line 180 feet in length. It is quite certain, from my own previous observations and those since made by M. Agassiz's directions on the glacier of the Aar, that the movement thus shown to have continued seventeen days without a *saltus* would have continued the whole season in the same manner. In fact, the *deformation* or flexure thus observed being sufficient to account for the whole excess of the central above the lateral motion, is in itself an explanation, and a proof that the explanation is adequate, and leaves nothing residual to be accounted for by *saltus*†.

I have more to add on this subject, but shall first give an account of an extension of this experiment on the actual flexure of the ice, upon so elaborate a scale as I scarcely ventured to hope would prove successful, especially as the time I could devote to watch its progress was small, and the circumstances of weather excessively unfavourable.

Having succeeded so well with the thirty feet station in the transverse line, I thought of multiplying the points of observation still further, so as to obtain a polygon of flexure more nearly approaching to a curve. This I did by making the first ninety feet of the transverse line, *i. e.* the space between Q and (3), Plate VIII. fig. 3, the subject of more immediate experiment, fixing in it forty-five stations only two feet apart. After several partial failures, which gave me, notwithstanding, encouraging results, I selected this plan. A space a foot wide and ninety feet long was cleared with hatchets and ice tools, so as to arrive at a nearly even surface of the hard delicately veined ice; and gutters were made so as to drain as far as possible the surface water from the part under experiment. The theodolite being stationed as usual over Q, and the vertical wire of the telescope describing a great circle passing through the line QQ1 transverse to the glacier, an assistant (Balmat), directed by my signals, bored

* See proofs cited in my Ninth Letter on Glaciers, Edinburgh Philosophical Journal, April 1845, and in second edition of Travels in the Alps, Appendix.

† Mr. Williamson, Fellow of Clare Hall, Cambridge, to whom I proposed this experimental test of the theory of movement by *echelons*, made a series of independent observations on the Mer de Glace, which coincided in result with what has been stated above. After a patient examination of these facts, and of others which he observed on different glaciers, I am glad to say that Mr. Williamson was led to abandon the theory of sliding columns or fragments, and to accept that of plasticity as connected with the mechanism of the veined structure which I have endeavoured to illustrate above.

a series of holes from two feet to two feet, forty-five in number, with a common carpenter's centre-bit, and as nearly as possible in the visual line. The holes, which were $\frac{4}{16}$th of an inch in diameter and about five inches deep, were immediately occupied by wooden pins prepared for the purpose. These pins were placed as nearly as possible in the visual straight line, but from the nature of the operation some errors were inevitable. The amount of these errors of position or zero of the marks was immediately determined by causing the vertical wire again to traverse the series, the assistant placing over the centre of the head of each pin in succession the zero point of a scale of inches divided both ways, and held parallel to the length of the glacier, so that (the divisions to tenths of an inch being very plainly marked, and divisible by estimation by the telescope) the fundamental position of each pin was determined, and considered as + if in advance of the transverse line (in the direction of the glacier's motion), and − if behind it (or nearer the origin of the glacier). The mere error of reading did not in any case exceed $\frac{1}{10}$th of an inch, though the uncertainty of centring of the theodolite over Q might amount to $\frac{1}{10}$th of an inch, or even more. The two marks nearest Q had their positions determined by a thread stretched from the station-pointer of the theodolite to the third mark, their distance being too small to be distinctly seen by the telescope.

The very same process, as regards the placing the zero of the scale on the head of the pin and reading off, was repeated on subsequent days, and the new readings *minus* the fundamental readings gave the *apparent* relative motion in the interval. This apparent motion had to be corrected, exactly as before explained, for the rotation of the visual line due to the translation of the fundamental point Q.

The following Table contains—(1.) the original readings on the four days of experiment, namely—

1844. August 20. 10 A.M.
August 21. 6 P.M.
August 23. 1 P.M.
August 26. 11 A.M.

(2.) The differences from the fundamental readings or total apparent displacements for each day, reckoning from the commencement. (3.) The same corrected for the rotation of the visual line from the following data:—

| August. | Interval. days. | Motion of Q. inches. | Correction at dist. 90 feet. inches. |
|---|---|---|---|
| 20 to 21. | 1·33 | 16 | 0·56 |
| 20 to 23. | 3·12 | 38 | 1·33 |
| 20 to 26. | 6·08 | 73 | 2·56 |

TABLE IV.—Showing the Apparent and True relative motions of forty-five points two feet apart, in a line transverse to the axis of the Mer de Glace, 1844.

| | Readings of Position. | | | | Apparent Motion. | | | Corrected Motion. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Mark, No. | Aug. 20, 10 A.M. | Aug. 21, 6 P.M. | Aug. 23, 1 P.M. | Aug. 26, 11 A.M. | August 20, 21. | August 20–23. | August 20–26. | August 20, 21. | August 20–23. | August 20–26. | Remarks. |
| 1. | +0·15 | −0·1 | −0·25 | 0·0 | −0·25 | −0·4 | − 0·15 | −0·25 | −0·45 | −0·15 | Between 1 and 2 a slight fissure. |
| 2. | +0·3 | −0·5 | −0·25 | − 0·2 | −0·8 | −0·55 | − 0·5 | −0·9 | −0·6 | −0·55 | |
| 3. | +0·15 | +0·15 | +0·6 | + 0·7 | 0·0 | +0·45 | + 0·55 | −0·05 | +0·35 | +0·4 | |
| 4. | −0·2 | −0·1 | +0·25 | + 0·3 | +0·1 | +0·45 | + 0·5 | +0·05 | +0·35 | +0·3 | |
| 5. | −0·25 | −0·35 | −0·05 | + 0·3 | −0·1 | +0·2 | + 0·55 | −0·15 | +0·05 | +0·25 | |
| 6. | −0·25 | 0·0 | +0·6 | + 1·2 | +0·25 | +0·85 | + 1·45 | +0·2 | +0·65 | +1·1 | |
| 7. | −0·2 | +0·2 | +0·9 | + 1·8 | +0·4 | +1·1 | + 2·0 | +0·3 | +0·9 | +1·6 | Veined structure very strong. |
| 8. | 0·0 | +0·3 | +1·2 | + 2·45 | +0·3 | +1·2 | + 2·45 | +0·2 | +0·95 | +2·0 | |
| 9. | 0·0 | +0·5 | +1·45 | + 2·5 | +0·5 | +1·45 | + 2·5 | +0·4 | +1·2 | +2·0 | Between 9 and 10 a fissure. |
| 10. | −0·2 | +0·15 | +1·1 | + 2·3 | +0·35 | +1·3 | + 2·5 | +0·25 | +1·0 | +1·95 | |
| 11. | −0·4 | +0·25 | +1·35 | + 2·7 | +0·65 | +1·75 | + 3·1 | +0·5 | +1·4 | +2·45 | |
| 12. | −0·0 | +0·5 | +1·6 | + 2·85 | +0·5 | +1·6 | + 2·85 | +0·35 | +1·25 | +2·15 | |
| 13. | −0·55 | +0·15 | +1·45 | + 3·3 | +0·7 | +2·0 | + 3·85 | +0·55 | +1·6 | +3·1 | |
| 14. | −0·55 | +0·35 | +1·5 | + 3·7 | +0·9 | +2·05 | + 4·25 | +0·75 | +1·65 | +3·45 | |
| 15. | −0·15 | +0·6 | +2·05 | + 4·15 | +0·75 | +2·2 | + 4·3 | +0·55 | +1·75 | +3·45 | |
| 16. | −0·2 | +0·65 | +2·2 | + 4·5 | +0·85 | +2·4 | + 4·7 | +0·65 | +1·9 | +3·8 | |
| 17. | −0·25 | +0·6 | +2·2 | + 4·15 | +0·85 | +2·45 | + 4·4 | +0·65 | +1·95 | +3·45 | |
| 18. | −0·15 | +0·7 | +2·4 | + 4·85 | +0·85 | +2·55 | + 5·0 | +0·65 | +2·0 | +4·0 | |
| 19. | −0·1 | +0·95 | +2·5 | + 5·35 | +1·05 | +2·6 | + 5·45 | +0·8 | +2·05 | +4·4 | |
| 20. | −0·15 | +1·0 | +3·0 | + 5·9 | +1·15 | +3·15 | + 6·05 | +0·9 | +2·55 | +4·9 | Two fissures between 19–20 and 20–21. |
| 21. | +0·25 | +1·7 | +3·55 | + 6·7 | +1·45 | +3·3 | + 6·45 | +1·2 | +2·7 | +5·25 | |
| 22. | +0·1 | +1·6 | +3·5 | + 6·7 | +1·5 | +3·4 | + 6·6 | +1·25 | +2·75 | +5·35 | |
| 23. | +0·3 | +1·7 | +4·0 | + 7·55 | +1·4 | +3·7 | + 7·25 | +1·1 | +3·0 | +5·95 | 22–23, slight fissure very oblique. |
| 24. | +0·2 | +1·65 | +4·0 | + 7·9 | +1·45 | +3·8 | + 7·7 | +1·15 | +3·1 | +6·35 | |
| 25. | −0·3 | +1·15 | +3·5 | + 8·0 | +1·45 | +3·8 | + 8·3 | +1·15 | +3·05 | +6·9 | |
| 26. | +0·35 | +1·7 | +4·3 | + 7·95 | +1·35 | +3·95 | + 7·6 | +1·05 | +3·2 | +6·1 | 26–27, a slight fissure. It recrosses the |
| 27. | +0·25 | +1·75 | +4·4 | + 8·45 | +1·5 | +4·15 | + 8·2 | +1·2 | +3·35 | +6·65 | line at 40–41. |
| 28. | +0·25 | +2·05 | +4·9 | + 9·1 | +1·8 | +4·65 | + 8·85 | +1·45 | +3·8 | +7·25 | |
| 29. | +0·4 | +2·2 | +5·15 | + 9·35 | +1·8 | +4·75 | + 8·95 | +1·45 | +3·9 | +7·3 | |
| 30. | +0·15 | +1·9 | +4·9 | + 9·3 | +1·75 | +4·75 | + 9·15 | +1·4 | +3·85 | +7·45 | |
| 31. | 0·0 | +2·0 | +4·7 | + 9·0 | +2·0 | +4·7 | + 9·0 | +1·65 | +3·8 | +7·25 | |
| 32. | +0·2 | +2·3 | +5·3 | + 9·7 | +2·1 | +5·1 | + 9·5 | +1·7 | +4·15 | +7·7 | |
| 33. | +0·2 | +2·1 | +5·5 | +10·0 | +1·9 | +5·3 | + 9·8 | +1·5 | +4·3 | +7·95 | |
| 34. | +0·9 | +2·9 | +6·0 | +10·95 | +2·0 | +5·1 | +10·05 | +1·6 | +4·1 | +8·15 | |
| 35. | +0·25 | +2·2 | +5·7 | +10·5 | +1·95 | +5·45 | +10·25 | +1·55 | +4·4 | +8·25 | 34–35, a very slight fissure. |
| 36. | +0·1 | +2·1 | +5·5 | +10·15 | +2·0 | +5·4 | +10·05 | +1·55 | +4·35 | +8·0 | |
| 37. | −0·1 | +2·05 | +5·3 | +10·5 | +2·15 | +5·4 | +10·6 | +1·7 | +4·3 | +8·5 | |
| 38. | 0·0 | +2·1 | +5·9 | +11·3 | +2·1 | +5·9 | +11·3 | +1·65 | +4·75 | +9·15 | |
| 39. | +0·2 | +2·4 | +6·35 | +11·8 | +2·2 | +6·15 | +11·6 | +1·75 | +5·0 | +9·4 | |
| 40. | 0·0 | +2·3 | +5·9 | +11·1 | +2·3 | +5·9 | +11·1 | +1·8 | +4·7 | +8·85 | |
| 41. | +0·1 | +2·5 | +6·4 | +12·15 | +2·4 | +6·3 | +12·05 | +1·9 | +5·1 | +9·7 | 40–41. See 26–27. |
| 42. | 0·0 | +2·3 | +6·25 | +11·8 | +2·3 | +6·25 | +11·8 | +1·8 | +5·0 | +9·4 | |
| 43. | −0·1 | +2·2 | +6·15 | +11·65 | +2·3 | +6·25 | +11·75 | +1·8 | +4·95 | +9·3 | |
| 44. | 0·0 | +2·4 | +6·5 | +12·35 | +2·4 | +6·5 | +12·35 | +1·85 | +5·2 | +9·85 | |
| 45. | −0·2 | +2·4 | +6·1 | +12·25 | +2·6 | +6·3 | +12·45 | +2·05 | +4·95 | +9·9 | |

The results of these observations will be best understood by consulting the projected results in Plate IX. fig. 2, in which the advance of the marks is magnified twofold relatively to the spaces between them, which was necessary for the reduced scale of the engraving, although it has the disadvantage of increasing the deviations from symmetry which might arise from errors of observation. As those who have not attempted the actual execution of such experiments cannot be aware of the difficulties they entail, it may be just to mention that during weather so unfavourable as that which occurred

during the continuance of these experiments, nothing can be so irksome as the necessity of persevering in the face of physical obstacles, the only alternative which the necessary limitation of my stay afforded being to abandon them. I do not speak of the painful effort of conducting delicate observations for hours under a hot sun, whilst the feet are immersed in the liquid sludge of decaying snow, for I am not aware of having sacrificed the precision of a single observation to such a cause (though in the course of my glacier experience I have sometimes been compelled to abandon or discontinue observations), but it is easy to see that the success of experiments like this depended upon the absolute fixity of the marks inserted in the unstable and wasting surface of the glacier, and that the most dry and uniform condition of the ice seemed alone to promise a chance of finding the small pins in the exact positions in which they had been planted a day or two before. Instead of this, eleven out of nineteen days which I spent at Chamouni were wet, and notwithstanding the season of the year, the glacier was repeatedly covered with snow, which in melting under a succeeding fierce sun, left the surface honeycombed by infiltration and streaming with wet, so that the preservation of the holes was only effected by laboriously covering every one with large flat stones during the intervals of observation, and even this was not free from other disadvantages which it would take too long to particularize. On the whole, the uniformity of the triple curves exhibited in fig. 2 is surprising, considering the local errors to which the fixation of the pins was liable, and the smallness of the quantities sought. The first two curves, those for the 21st and 23rd August, are indeed as perfectly regular as it would be possible to expect from this kind of observation, even much more so than I had ever hoped to attain; but on the 26th the holes containing the pins were more degraded, and some manifest errors have arisen from this cause, and evidently affect only single marks, such as the twelfth and twenty-fifth mark, which singly have inclined forward or backward by the fusion of the ice. With these preliminaries as to the reasons why the irregularities of these curves should be judged with indulgence, I will state briefly the most apparent general results.

*First.* The flexure of the ice is proportional, almost exactly to the time elapsed in the intervals of the observations; and it is also graduated from point to point, not *staccato*, as would inevitably have been the case had the relative motion been due to the sliding of finite portions past one another, as in Plate VII. fig. 3. We perceive nothing of the kind.

*Secondly.* A singular peculiarity strikes the eye, which at first puzzled me, but when the cause was explained, confirms in no slight degree the accuracy of the methods employed and their fitness to reveal the minutest motion of the glacier. The curves of Plate IX. fig. 2, cut the axis, not all exactly at the same point; but on an average this point may be fixed with tolerable accuracy between the third and fourth mark, or seven feet from the fundamental station Q. *The first and second marks moved evidently slower than the point Q, or the zero*, and the progress of the third and fourth is dubious or irregular. The cause of this peculiarity was clearly ascer-

tained *on the spot* to be the existence of two crevasses belonging to the lateral system of crevasses, between which at their thinning out the station Q had been placed, under the idea that their distance was sufficiently great not to affect the motion. The position of these crevasses is shown in Plate VIII. fig. 3, by which it will be seen that they were fifty feet apart in the direction of the length of the glacier, and that a line joining their extremities passed eight feet nearer the centre of the glacier than the point Q, thus almost coinciding with the point of contrary flexure, which was evidently occasioned by a slightly superior advance of the mass of ice on which Q was placed, thus insulated in some degree between the two fissures. This enables us to transfer the origin of our curve to a point of undoubted solidity, namely, the fourth station, from which point it swells with the regularity which has been described, and which establishes the compactness of the ice experimented on in the most convincing manner.

*Thirdly.* The curves reckoned from the origin, thus experimentally indicated, show in a beautiful manner the convexity in the direction of the glacier motion before alluded to, which is singularly striking, considering the shortness of the space in which it is developed with nearly mathematical precision, being only about $\frac{1}{15}$th of the breadth of the glacier in this place (see ground plan, Plate VIII. fig. 1.). Even an inspection of the curves (Plate IX. fig. 2) can faintly convey the impression made upon my own mind, when upon the 26th of August I placed the theodolite for the last time over station Q, and caused the vertical wire to pass in front of the line of pins bent into the convex shape by the relative motion of six days' continuance. Thus seen in foreshortened perspective, the eye would in an instant have seized an abrupt motion or discontinuity of the line, but "the appearance of the curve they formed was beautiful; the whole line of pins was deviated from the usual line QQ1 by an angle equal to 12·45 inches, seen at a distance of ninety feet, or about 40′, and besides this, the pins lay in a beautiful and nearly continuous curve, presenting its convexity towards the valley, and decidedly without any great step or start. This was beautifully seen when I directed the vertical wire of the theodolite upon the forty-fifth pin and caused it to describe a vertical plane*. I observed however a curious fact, plainly indicated by the numerical results; the curve crossed the axis at the fourth pin, and attains its greatest convexity at the twenty-fifth†."

*Fourthly.* That no information might be wanting as to the precise condition of the mass of ice under experiment, I made a very minute examination of the state of the transverse line with respect to the occurrence of *flaws* in the ice. The most important of these was one which returned into itself, crossing the line towards the origin of the glacier between the twenty-sixth and twenty-seventh marks, and returning backwards between the fortieth and forty-first without extending further upwards. Such a flaw, even if devoid of cohesion, could only act by allowing the piece of ice contained between the twenty-seventh and fortieth mark to slip bodily forwards,

* An approximation to this effect will be obtained by stretching a fine thread over the figure.

† From my notes made at the time.

leaving a vacuity behind. No such slip is recorded in the observations, and no such gap or vacuity was left during the continuance of the observations for seventeen days. Nothing approaching to an open fissure occurred in any part of the space of ice under observation, and the few flaws noted (see the column of remarks in Table IV., and the indications of them by dotted lines and arrows in Plate IX. fig. 2, showing their directions) were perfectly close, and all more or less of zigzag forms, preventing the possibility of sliding. The *veined structure* was developed in every part of the section, in some parts more admirably than others, as near the seventh mark, and between the fortieth and forty-fifth. These countless blue veins may be considered as so many flaws or partial solutions of continuity, existing or having existed; they are almost perfectly *straight*, and (as will be shown immediately) *exactly* in the direction in which the relative motion of the parts of the ice demonstrated by these experiments takes place. It would require strong evidence to convince us that these veins are not occasioned by, and the mechanism of, the plastic motion of the ice.

The beautiful convexity of the curves in the direction of motion which the eye at once seized, and could more accurately distinguish by the theodolite, as having its largest sagitta at the twenty-fifth mark, namely, almost exactly midway between the fourth mark or convex origin, and the forty-fifth or extreme mark, contains in itself an evidence with which no person of correct habits of thought can fail to be struck as proving a regular plastic action of gravity or other propelling force acting from point to point on the mass of the glacier. I made however a check experiment almost unnecessary, but which I will here detail. Lest it should be alleged that the whole area under experiment was a moving one capable of being *swung* round by the pressure upon the centre of the glacier, so that the displacement of the transverse line was due to rotation of the mass operated on round some distant centre, I took care near the commencement of my experiment to fix a mark in the ice in the same line with Q parallel to the length of the glacier, or perpendicular to the transverse visual line at the commencement of the experiment. This point is marked $q$ in Plate VIII. fig. 3. Now had the block of ice on which the marks Q, $q$, (1), (2), (3), &c. were fixed, been revolving round some fixed or moveable centre, the right angle (3) Q $q$ would have remained a right angle, and so for (2) Q $q$ and (1) Q $q$. But these angles constantly became more obtuse, and that in different degrees depending on the convexity of the curve Q, (1), (2), (3): and so of (4) and (5). It is impossible, therefore, to avoid the conclusion, that the solid ice was itself *distorted* to the amount of the excess of progression of the more central above the lateral stations.

The results are contained in the following Table, the five stations being those formerly mentioned at 30, 60, 90, 120, and 180 feet from Q. The first three stations were fixed when the visual line had an azimuth of 89° 55′ from $q$; the fourth was fixed on the 14th of August, when the visual line had revolved through 3′ (its daily progression towards $q$ in consequence of the translation of the station Q being 1′ 30″ exactly), and the fifth on the 17th, when the visual line was in azimuth 89° 47½′.

TABLE V.—Showing the variable azimuths (observed) of the transverse stations with the longitudinal direction Q*q*.

| Date. | (1). | (2). | (3). | (4). | (5). | Angle Q1 Q *q*. |
|---|---|---|---|---|---|---|
| | ° ′ | ° ′ | ° ′ | ° ′ | ° ′ | ° ′ |
| August 12. 2 P.M. | 89 55 | 89 55 | 89 55 | ..... | ..... | 89 55 |
| 14. 1 P.M. | 90 6½ | 90 5 | 90 5 | 89 52 | | |
| 17. Noon. | 90 26 | 90 24 | 90 23 | 90 8 | 89 47½ | |
| 19. 5 P.M. | 90 47 | 90 46 | 90 44 | 90 10 | 89 52 | |
| 21. 6 P.M. | 90 56 | 90 56 | 90 53 | 90 21 | 90 3 | |
| 23. 1 P.M. | 91 9½ | 91 9½ | 91 4½ | 90 34 | 90 14 | |
| 26. Noon. | 91 24 | 91 23 | 91 18 | 90 45 | 90 24½ | 89 28½ |

In the last column is the observed azimuth of the visual line joining the fixed point Q1 with the moveable station Q, from the mark *q* in the glacier, on the first and last observation. This angle had therefore diminished by 26′½. Now from the known progression of Q, and the known distance QQ1, we computed at page 166 the rotation of the visual line, and found it to be 25′ 54″. This close correspondence is highly satisfactory, as showing that the relative positions of Q and *q* remained unchanged during the continuance of the experiment with reference to the imaginary transverse line by which they were adjusted. It is worthy of notice that the line Q*q* was exactly in the direction of the veined structure.

*Edinburgh, July* 1845.

# XIV. *Illustrations of the Viscous Theory of Glacier Motion.*—Part III.

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§ 6. *On the Motion of Glaciers of the Second Order.*
§ 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.*
§ 8. *Summary of the Evidence adduced in favour of the Theory.*

## § 6. *On the Motion of Glaciers of the Second Order.*

UP to the year 1844 no attempt had been made, so far as I am aware, to measure the rate of motion of those comparatively small isolated glacial masses reposing in the cavities of high mountains, or on *cols*, called by DE SAUSSURE *Glaciers of the Second Order.*

Some observations had indeed been made upon a glacier of this description in 1841, and by MM. MARTINS and BRAVAIS, during a residence on the Faulhorn. But it was not at that time known that the motion of glaciers was a continuous and regular one, admitting of rigorous measurement even in short intervals of time, and the importance of such observations was overlooked. They accordingly believed that the glacier in question had no sensible motion, and probably they did not attempt to observe it until a subsequent year. It is impossible now to doubt that the *Blau Gletscher*, near the Faulhorn, has a movement like all other bodies of the kind.

In July 1844, I had an opportunity of passing some days at the hospice of the Simplon, in the neighbourhood of which exists a small glacier of the second order, easy of access, and very fit for the experiment which I proposed to myself upon such bodies. Its diminutive size made it all the more suitable; for should it be found to possess a regular motion, we are certain that the *mechanism of a glacier* is continued within the small compass of a mass which may be conveniently examined in detail in all its parts. It is lodged in a niche of the mountain called the Schönhorn*, immediately behind the Simplon hospice: we shall therefore call it the glacier of the Schönhorn. From its inconsiderable extent, it might easily be overlooked by a passing traveller amidst the multitude of vast and striking objects by which he is surrounded†. It is perched, as has been said, in a kind of niche on the northern

* Also called Hübschhorn, an equivalent epithet.

† The reader will not for a moment imagine that it is the Kaltwasser glacier of which we speak, which lies also in the neighbourhood of the Schönhorn, descending from the Monte Leone and Wasenhorn, and from which the *Galerie du Glacier* on the Simplon road takes its name.

face of the Schönhorn, somewhat about an hour's steep climb above the hospice; consequently about 1400 feet higher. The hospice is itself 6580 English feet above the level of the sea; the mean height of the Schönhorn glacier may be taken at 8000 feet. I had not an opportunity of ascertaining it more accurately.

Plate X. figs. 1 and 2, shows a sketch of a front view of the Schönhorn taken from the opposite heights, and a ground plan of the glacier. The latter is sketched merely by the eye, but the scale is furnished by some actual measures. I first visited the glacier on the 20th of July 1844. It was then covered over, by far the greater part of its extent, with snow, as shown in the plan. This snow is in great part manifestly permanent, and the glacier is therefore in the state of *névé*. The general slope is from top to bottom of the plan, and its inclination is variable, depending upon the direction of the avalanches by which it is fed, of which the principal descends the rapid *couloir* marked C, when the inclination is about 35°. This avalanche forms a sort of ridge down the glacier, as indicated by the shading of the map, leaving a considerable space comparatively flat to the eastward. On the west, the snow thins off from the ridge until it exposes the ice near the part marked B, where the slope is still considerable, being 20°, and here we have the real mass of the glacier exposed, although the ice is not of an exceedingly hard or crystalline character. The front or lower termination of the glacier all along presents a steep, nearly precipitous surface of ice, sloping from 45° to 60°. This ice rests on a bed of debris of rock which appears to be inclined about 25°. Except near the precipitous termination of the glacier, there are no apparent crevasses. The surface is uniform and uninterrupted. Some water issues from beneath the steepest part of the ice; but even in the middle of the day, near the end of July, there was exceedingly little. The length, if it may be so termed, of the glacier, from back to front is about 1000 feet, and its greatest breadth 1300 feet. Its surface may be roughly estimated at twenty-six acres.

The rock of which the Schönhorn is composed, is an alternation of the slaty rocks resembling gneiss with talc slate, which are so common in this part of the Alps. To my great surprise, on one of my visits, I heard the sound of hammers and blasting in this elevated and remote spot; and found two men employed in quarrying Potstone (*Lapis ollaris*) for building ovens, from a retired nook beyond the glacier; the quarry is marked on the plan at E.

On the 20th of July 1844, I ascended to the glacier, accompanied by M. Alt, one of the clerical members of the Simplon establishment, and an assistant; and I fixed upon a position, marked St. on the rock on the east side of the glacier, for planting the instrument, which was then directed, as nearly as I could judge, in a line transverse to the prevailing slope of the glacier, and the telescope was made to describe a vertical plane. It was then sighted upon a well-marked quartz vein on the rock on the distant side of the glacier, marked D, by which it could at any time be brought into precisely the same position; the position of the instrument itself being

referred to a mark cut on the rock where it stood. Two marks were then fixed on the glacier; one was a pole stuck in at A, several feet into the snow of the avalanche already described as traversing the length of the glacier. The slope of the snow at the point A was about 10°; and the distance of A from the station St., by an approximate measurement, 340 feet. 350 feet further in the same direction, a hole was made with a blasting iron into the solid ice at B, where the inclination was 20°. The precise position of these marks being determined relatively to the visual line, the observation was finished at 4 o'clock P.M.

On the 23rd of July we returned. The mark A in the snow (which was so firmly driven in that it could not be withdrawn without breaking the pole) had advanced in the direction of the slope exactly four inches at 1 P.M., or in sixty-nine hours; whilst the mark B in the ice had advanced $5\frac{1}{4}$ inches in the same time; whence we have

Velocity of A in twenty-four hours . . . . 1·4 inch.
Velocity of B in twenty-four hours . . . . 1·8 inch.

The result was what I had anticipated, although it must be confessed it might be expected to be nearly the same upon any theory of glacier motion yet proposed. The slope of a glacier, *per se*, is not an index of what should be the velocity of motion on the viscous theory. No doubt, other things being equal, the velocity will be proportional to some function of the declivity, and such we have seen to be fully borne out by experiments on the Mer de Glace of Chamouni; and in the present case, the velocity under a slope of 20° was about one-third greater than that under a slope of 10°. But the analogy of a river, as well as theoretical considerations, show that the slope is but one of numerous considerations; such as (1) the *mass* of the viscous body; the smaller the mass the smaller the velocity on a given slope*; (2) the state of infiltration or wetness of the glacier altering its resistance to change of form†. Without mentioning other causes, these are quite sufficient to account for the small velocity observed, when we recollect the very insignificant mass of this glacier and its dry state arising from its great elevation, its northern exposure, and even the very inclination of its bed which keeps it in a state of perfect drainage and leaves it always in a state tending to the *snowy*, rather than that of imbibition.

### § 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.*

The first estimate of the least authority on the advance of any point of a glacier from year to year, was made by Hugi on the glacier of the Aar, from 1827 to 1836. The method employed was to measure the distance of a well-marked block of stone, resting on the ice from a transverse line determined by the fixed objects on the shore. This is the only way, generally speaking, practicable upon glaciers at a distance from habitations, and where marks cannot be conveniently renewed in the ice from time to time during the whole year. The velocity of the part of the glacier imme-

* Travels in the Alps of Savoy, 2nd edit., p. 387. † Ibid. pp. 148. 371.

diately below the promontory, called the Abschwung, was found to be about 240 feet per annum, which though neither confirmed nor invalidated by the discordant measurements subsequently made by other observers on the same glacier, has at length been substantially corroborated by a professional surveyor, M. Wild, who has recently undertaken the verification at M. Agassiz's request.

After having myself observed the motion of several points of the Mer de Glace of Chamouni during the summer of 1842, I fixed the positions of two conspicuous blocks, one near Montanvert, marked D 7; and another opposite the Tacul, marked C, or the Pierre Platte (see my Map of the Mer de Glace), by means of which I hoped to ascertain the mean annual motion in succeeding years. With respect to the latter, or the Pierre Platte, I was successful; for in September 1843 I ascertained geometrically its change of position, subject however to the uncertainty of a few yards, owing to the sliding of the block from the pedestal of ice upon which it was so picturesquely poised*, a circumstance which happens once or twice in the course of every summer.

From the 17th of September 1842, to the 12th of September 1843,
the advance was (in 360 days) . . . . . 256·8 feet.
Or reduced to the exact year of 365 days . . . . . 260·4 feet.
Mean daily motion . . . . . 8·56 inches.

Again, being enabled to repeat the measures in 1844, I found the advance—
From the 12th of September 1843, to the 19th of August 1844 (342 days) 270 feet.
Proportional motion for 365 days . . . . . 288·3 feet.
Mean daily motion . . . . . 9·47 inches.

In the case of the block D 7, I was less fortunate. It was very near the western side of the glacier, and though not thrown up on the shore, yet the ice on which it rested got in some manner so embayed or entangled, that though its motion had been steadily watched during the winter of 1842–43 by my able assistant, Auguste Balmat, it had scarcely moved since his last observation on the 8th of June 1843, when I visited it in September of the same year. It must be presumed that it had been much retarded previously, and hence it is clearly inadmissible to infer a proportional motion for the portion of the year when it had not been observed, as I did in the Postscript at the end of the first edition of my Travels, whilst in ignorance of the then unsuspected retardation. The motion actually observed was 432 feet in 322 days, being at the rate of 483 feet per annum, or 15·88 inches per day. This is therefore undoubtedly *below* the true measure of the annual motion of the side-part of the glacier somewhat in advance of the Châlet of Montanvert (see the position of D 7 in the Map). It may at least be of some service as an *inferior limit* of the annual motion there.

In 1843 I fixed approximately the position of a block marked P, higher up the glacier than the Montanvert, and near its left bank, exactly opposite the spot called Les Ponts.

* See Frontispiece to Travels through the Alps of Savoy.

The observation, being repeated the ensuing year, gave a motion of *about* 486 feet (the nature of the observation did not admit of the same accuracy as at station C) from the 13th of September 1843 to the 9th of August 1844, or 331 days, being at the rate of

536 feet per annum,

or 17·62 inches per day.

In 1844 I made the casual discovery of one of my staves, used to mark the position of the station A at the *Angle*, a little higher up the glacier than the last, a point of which the motion had been most carefully observed during the summer of 1842 (see Travels, p. 140). This stick still bore legibly written upon it the date when it had been fixed in the ice at station A, and as the painted marks on the rock of the *Angle* were still as fresh as when they were made, I had no difficulty in finding the exact position on the glacier which this mark had in any part of the summer of 1842, and by measuring the distance to the place where it was found (which was on a spot of the ice quite unfrequented by guides or any one else), I had good reason for believing that this must be the space over which it had travelled in the mean time; although of course I do not ascribe to this observation the weight of a direct measure, yet it proves an interesting confirmation. Reckoning from the position it occupied on the 1st of September 1842, it had advanced down to the 26th of August 1844, or in 720 days . . . . . 952 feet,

or, per annum . . . . 482·5 feet.

Mean daily motion . . . 15·87 inches.

It will be seen that this result is in close agreement with that observed at station P above mentioned, which is a little further down the glacier, but about the same distance from the side; for though the motion of P is somewhat greater for 1843–44 than the mean motion of A for 1842–44, it will be seen by the comparative observations at C already referred to, that the glacier moved more rapidly in 1843–44 than in 1842–43.

But I am now enabled to present a view of the actual progress of two glaciers during every part of the year from direct observation. For these I am indebted to the intelligent and persevering zeal of my excellent guide and assistant at Chamouni, Auguste Balmat, of whose character I have had the pleasure of forming a more and more favourable estimate the longer I have been acquainted with him. To the long training of the laborious summer of 1842, when he assisted me, he adds the further experience derived from my visits in 1843 and 1844, in the latter of which especially he became familiar with the nice precautions requisite in conducting the most accurate measurements, and received instructions from me which rendered him perfectly competent to continue by himself the simpler kind of measurements which I have alone required of him. The extraordinary exertions which he used to obtain the winter motion of the block D 7, under the Montanvert, in 1842–43, have been noticed in my former publications. On one or two occasions, as I learned afterwards from himself, being unable to ascend the usual path to the Montanvert for fear of spring avalanches,

he actually clambered with a companion up the rugged ascent from the source of the Arveiron, plunging continually up to the middle in snow, for no other purpose than to make the observation which I had requested of him; and it would be unjust not to mention at the same time the admirable, because rare, generosity, with which he positively refused for himself any share of the remuneration which I pressed upon him the following summer, as some recompense for the fatigues and dangers which he had braved to obtain for me this information. With such a person, my confidence in the observations which he has since made at points much more accessible, and with the experience of some additional years, is complete. I do not mean that mistakes may not occur, or even that the measures may not be less exact than I might have taken myself; but from my knowledge of the man, I am nearly as confident in their *being faithfully reported, exactly as they were made*, as if I had done so myself.

With a view to lighten the labour as much as possible, I selected two stations on the *glacier of Bossons*, and desired Balmat to select two on the *Glacier des Bois* (the outlet of the *Mer de Glace* towards the valley of Chamouni); all these points being tolerably accessible at every season of the year.

The general method of observation was the following:—vertical holes were driven into the ice with a 4-foot blasting iron, at the points whose motion was to be determined; and these holes were renewed from time to time as the surface of the ice wasted. A staff of wood $5\frac{1}{2}$ feet long, was stuck in each, which projected sufficiently above the snow (which never appears to have exceeded $2\frac{1}{2}$ feet deep on the glacier) to make it visible at all seasons. During winter the staves were frozen into the ice, and the waste being small, the holes did not require renewal. Two marks are then made of a permanent kind on the rocks of the moraine, or two staves driven in, or a distant object on the farther side of the glacier was observed, so as to mark out sufficiently a line transverse to the glacier, the prolongation of which passes over the hole in the ice when first made; and the advance of the hole in the ice beyond this fixed visual line marks the progress of the glacier. The want of a theodolite is supplied by directing the eye past a plumb-line suspended over the fixed mark on the moraine nearest to the glacier, the eye of the observer being over the farthest mark. As the spaces moved over were in most cases considerable, an error of a few inches, or even a foot, is not important to the result. The progress was in every case determined by means of a line marked with *English* feet and inches, left by me at Chamouni on purpose.

The results were communicated to me regularly by letter at intervals of a few weeks during the whole year, and all questions asked and explanations required by me were answered by return of post.

Those who may look with suspicion upon observations made in a remote place by a peasant of the better class, though they may not partake of my security in the results from knowing the character of the individual, will, I believe, have their doubts removed by the internal evidence of this important series of observations,

which even a philosopher could not have invented, and which, it will be seen, are confirmed by data of quite another kind over which the observer could have no control, I mean the Meteorological Registers of Geneva and St. Bernard.

## Table I.

First Station on the Glacier des Bois, a little way below the Chapeau, and at about *one-third* of the breadth of the glacier from its eastern bank.

| | | | | | | Space moved over in English feet and inches. | | Daily motion. |
|---|---|---|---|---|---|---|---|---|
| From 1844 | | | To 1844. | | | feet. | inches. | inches. |
| October .. | 2. | 10 A.M. .... | October .... | 14. | 9 A.M. | 32 | 0 | 32·0 |
| October .. | 14. | 9 A.M. .... | November .. | 2. | 8 A.M. | 43 | 11 | 27·8 |
| November .. | 2. | 8 A.M. .... | November .. | 19. | 4 P.M.* | 34 | 11 | 24·2 |
| November .. | 20. | 1 P.M. .... | December .. | 4. | 3 P.M. | 13 | 10 | 11·8 |
| | | | 1845. | | | | | |
| December .. | 4. | 3 P.M. .... | January .... | 7. | 3 P.M. | 32 | 8 | 11·5 |
| 1845. | | | | | | | | |
| January .... | 7. | 3 P.M. .... | February .. | 18. | 3 P.M. | 49 | 2 | 14·0 |
| February .. | 18. | 3 P.M. .... | March .... | 18. | 2 P.M. | 39 | 10 | 17·0 |
| March .... | 18. | 2 P.M. .... | April ..... | 17. | 10 A.M. | 42 | 1 | 16·9 |
| April ..... | 17. | 10 A.M. .... | May ..... | 17. | 8 A.M. | 56 | 3 | 22·5 |
| May ..... | 17. | 8 A.M. .... | May ..... | 31. | 2 P.M. | 43 | 11 | 37·0 |
| May ..... | 31. | 2 P.M. .... | June ..... | 19. | 4 P.M. | 61 | 11 | 38·4 |
| June ..... | 19. | 4 P.M. .... | July ..... | 4. | 10 A.M. | 52 | 0 | 42·3 |
| July ..... | 4. | 10 A.M. .... | July ..... | 18. | 5 P.M. | 62 | 0 | 52·1 |
| July ..... | 18. | 5 P.M. .... | August .... | 6. | 4 P.M. | 77 | 6 | 49·0 |
| August .... | 6. | 4 P.M. .... | October .... | 8. | 4 P.M. | 187 | 8 | 35·7 |
| October .... | 6. | 9 A.M. (?) | November .. | 8. | 2 P.M. | 100 | 9 | 36·4 |
| November .. | 8. | 2 P.M. .... | November .. | 21. | 1 P.M. | 32 | 6 | 30·1 |

## Table II.

Second Station, Glacier des Bois, near the lowest extremity, just behind the "Côte du Piget."

| | | | | | | Space moved over in English feet and inches. | | Daily motion. |
|---|---|---|---|---|---|---|---|---|
| From 1844 | | | To 1845. | | | feet. | inches. | inches. |
| December .. | 4. | 2 P.M. .... | January .... | 7. | 4 P.M. | 8 | 6 | 3·3 |
| 1845. | | | | | | | | |
| January .... | 7. | 4 P.M. .... | February .. | 18. | 4 P.M. | 8 | 11 | 2·6 |
| February .. | 18. | 4 P.M. .... | March .... | 18. | 3 P.M. | 7 | 0 | 3·0 |
| March .... | 18. | 3 P.M. .... | April ..... | 17. | 11 A.M. | 11 | 7 | 4·6 |
| April ..... | 17. | 11 A.M. .... | May ..... | 17. | 9 A.M. | 18 | 4 | 7·3 |
| May ..... | 17. | 9 A.M. .... | May ..... | 31. | 1 P.M. | 10 | 5 | 8·8 |
| May ..... | 31. | 1 P.M. .... | June ..... | 19. | 2 P.M. | 13 | 3 | 8·3 |
| June ..... | 19. | 2 P.M. .... | July ..... | 4. | 9 A.M. | 14 | 1 | 11·1 |
| July ..... | 4. | 9 A.M. .... | July ..... | 18. | 6 P.M. | 17 | 6 | 14·6 |
| July ..... | 18. | 6 P.M. .... | August .... | 6. | 3 P.M. | 18 | 8 | 11·9 |
| August .... | 6. | 3 P.M. .... | October .... | 6. | 7 A.M. | 50 | 1 | 9·9 |
| October .... | 6. | 7 A.M. .... | November .. | 8. | 4 P.M. | 27 | 2 | 9·8 |
| November .. | 8. | 4 P.M. .... | November .. | 21. | 11 A.M. | 8 | 0 | 7·5 |

* There is some uncertainty about the circumstances of this observation, which from the difficulty of corresponding satisfactorily at so great a distance about minute local occurrences, I have been unable perfectly to clear up. It is probably correct as it stands.

## Table III.

First Station on the Glacier des Bossons. Some way above the Plateau, where the glacier is usually crossed; on the west side, and near the moraine.

| | | | | | | Space moved over in English feet and inches. | | Daily motion. |
|---|---|---|---|---|---|---|---|---|
| From 1844 | | | To 1844. | | | feet. | inches. | inches. |
| November.. | 20. | Noon. .... | December .. | 4. | 3 P.M. | 20 | 4 | 17·3 |
| | | | 1845. | | | | | |
| December .. | 4. | 3 P.M. .... | January .... | 7. | 1 P.M. | 44 | 11 | 15·9 |
| January .... | 7. | 1 P.M. .... | February .. | 14. | 4 P.M. | 46 | 9 | 13·6 |
| February .. | 17. | 4 P.M. .... | March .... | 17. | Noon. | 35 | 10 | 15·4 |
| March .... | 17. | Noon. .... | April ..... | 16. | 5 P.M. | 32 | 7 | 12·9 |
| April..... | 16. | 5 P.M. .... | May ..... | 17. | 3 P.M. | 60 | 0 | 23·3 |
| May ..... | 17. | 3 P.M. .... | May ..... | 31. | 8 A.M. | 49 | 0 | 42·9 |
| May ..... | 31. | 8 A.M. .... | June ..... | 19. | 10 A.M. | 54 | 2 | 34·1 |
| June ..... | 19. | 10 A.M. .... | July ..... | 4. | 5 P.M. | 53 | 8 | 42·1 |
| July ..... | 4. | 5 P.M. .... | July ..... | 21. | 2 P.M. | 43 | 1 | 30·6 |
| July ..... | 21. | 2 P.M. .... | August .... | 7. | 3 P.M. | 40 | 10 | 28·8 |
| August .... | 7. | 3 P.M. .... | October.... | 6. | Noon. | 103 | 0 | 20·6 |
| October.... | 6. | Noon. .... | November .. | 10. | 2 P.M. | 56 | 8 | 19·4 |
| November.. | 10. | 2 P.M. .... | November .. | 22. | 11 A.M. | 22 | 5 | 22·6 |

## Table IV.

Second Station on the Glacier des Bossons. Near the lowest extremity of the glacier, where free from the moraine, on the western side.

| | | | | | | Space moved over in English feet and inches. | | Daily motion. |
|---|---|---|---|---|---|---|---|---|
| From 1844 | | | To 1844. | | | feet. | inches. | inches. |
| October.... | 2. | 2 P.M. .... | October.... | 13. | 4 P.M. | 12 | 11 | 14·0 |
| October.... | 13. | 4 P.M. .... | October.... | 31. | 10 A.M. | 25 | 1 | 17·0 |
| November.. | 20. | 1 P.M. .... | December .. | 4. | 2 P.M. | 15 | 4 | 13·1 |
| | | | 1845. | | | | | |
| December .. | 4. | 2 P.M. .... | January .... | 7. | 3 P.M. | 36 | 11 | 13·0 |
| 1845. | | | | | | | | |
| January.... | 7. | 3 P.M. .... | February .. | 19. | 1 P.M. | 43 | 1 | 12·0 |
| February .. | 19. | 1 P.M. .... | March .... | 17. | 1 P.M. | 27 | 10 | 12·8 |
| March .... | 17. | 1 P.M. .... | April ..... | 16. | 6 P.M. | 25 | 8 | 10·2 |
| April..... | 16. | 6 P.M. .... | May ..... | 17. | 4 P.M. | 49 | 11 | 19·4 |
| May ..... | 17. | 4 P.M. .... | May ..... | 31. | 9 A.M. | 34 | 6 | 30·2 |
| May ..... | 31. | 9 A.M. .... | June ..... | 19. | 11 A.M. | 44 | 2 | 27·8 |
| June ..... | 19. | 11 A.M. .... | July ..... | 4. | 6 P.M. | 41 | 2 | 32·3 |
| July ..... | 4. | 6 P.M. .... | July ..... | 21. | 11 A.M. | 37 | 3 | 26·4 |
| July ..... | 21. | 11 A.M.* .... | August .... | 7. | 4 P.M. | 30 | 3 | 21·4 |
| August .... | 7. | 4 P.M. .... | October.... | 6. | 2 P.M. | 82 | 7 | 16·5 |
| October.... | 6. | 2 P.M. .... | November .. | 10. | 4 P.M. | 17 | 3 | 5·9 |
| November.. | 10. | 4 P.M. .... | November .. | 22. | 1 P.M. | 7 | 2 | 7·2 |

These four sets of observations are projected in Plate XI. fig. 1, where the four lower zigzag curves represent the gradation of diurnal velocity by periods, according to the method adopted in projecting my own observations in my Travels, p. 141. The

* Marked 4 P.M., perhaps by mistake, but computed on that supposition.

general accordance is sufficiently manifest, and the effect of the season of the year is beautifully shown, the following being the minimum and maximum values:—

| | Daily motion in inches. |
|---|---|
| Glacier des Bois, No. I., minimum in December | 11·5 |
| Glacier des Bois, No. I., maximum in July | 52·1 |
| Ratio of maximum to minimum | $4\frac{1}{2}$ : 1 |
| Glacier des Bois, No. II., minimum in January | 2·6 |
| Glacier des Bois, No. II., maximum in July | 14·6 |
| Ratio of maximum to minimum | $5\frac{1}{2}$ : 1 |
| Glacier des Bossons, No. I., minimum in March | 12·9 |
| Glacier des Bossons, No. I., maximum in May | 42·9 |
| Ratio of maximum to minimum | $3\frac{1}{3}$ : 1 |
| Glacier des Bossons, No. II., minimum in March | 10·2 |
| Glacier des Bossons, No. II., maximum in June | 32·3 |
| Ratio of maximum to minimum | $3\frac{1}{4}$ : 1 |

From these observations we may deduce the annual motion from November 1844 to November 1845 with considerable exactness. Allowing for the fractional parts of a year, we obtain the following results, amongst which I have included a separate computation of the mean daily motion for the summer period (April—October), and the winter period (October—April).

TABLE V.

| | Bois, No. I. | Bois, No. II. | Bossons, No. I. | Bossons, No. II. |
|---|---|---|---|---|
| | feet. | feet. | feet. | feet. |
| Motion for 365 days, November 1844 to November 1845.... | 847·5 | 220·8 | 657·8 | 489·1 |
| | inches. | inches. | inches. | inches. |
| Mean daily motion ..... | 27·8 | 7·3 | 21·6 | 16·1 |
| Mean daily motion, summer period, April to October ..... | 37·7 | 9·9 | 28·0 | 22·2 |
| Mean daily motion, winter period, October to April ..... | 19·1 | 4·7 | 15·8 | 10·7 |
| Ratio, summer: winter, motion..... | 2·0 : 1 | 2·1 : 1 | 1·8 : 1 | 2·1 : 1 |

I. From this Table we deduce in the first place a mean annual motion far greater than has hitherto been observed, or perhaps suspected in any glacier, that of near 300 yards, or almost *one sixth* of a mile. This is on the Glacier des Bois beneath the *Chapeau*, where the inclination of the glacier is very steep, adding a new illustration of the general principle*, that in *similar* circumstances the velocity increases with the slope. To this cause may be added the high temperature of the air of the valley to which in this part of its course it is exposed; but this last cause is alone insufficient; for

II. We find that the lowest part of the same glacier immediately behind the Côte

* Travels, 2nd edit. p. 371.

du Piget, a little way above the source of the Arveiron, and therefore still deeper in the valley, has a mean velocity nearly *four times less*, arising solely from the diminished slope and volume of the glacier in that part*. Hence there must be a condensation of the ice here, a pressure *à tergo*, the quicker moving ice pressing against the slower, consolidating it, remoulding its plastic material and sealing the crevasses; and a slight examination of the state of the glacier at the points in question will show this to be the case.

III. All that has now been said with respect to the two stations on the Glacier des Bois may be repeated with only numerical differences with respect to the two stations on the Glacier des Bossons; the one set of observations confirming the other.

IV. In both glaciers the summer motion exceeds the winter motion in a greater proportion, as the station is lower, that is, exposed to more violent alternations of heat and cold; this we shall find to be general.

Before continuing our deductions, we would call attention to the close relation which may be established between the mean temperature of any portion of the year and the velocity of the glacier corresponding to it. This is done in figure 1, Plate XI., exactly in the same way as I did when comparing my observations in the summer of 1842 with the corresponding changes of temperature†. That is to say, I have projected by *periods* (corresponding to the intervals of observation on the glaciers) the mean temperatures as observed at Geneva and at the Great St. Bernard, which are regularly published in the *Bibliothèque Universelle*, the average of which (separately deduced from the mean of daily maxima and minima, and projected in the upper part of the figure) may represent not inaptly the average temperature to which the glaciers in question, and especially the middle and lower regions of them, are exposed; and further, this average possesses the advantage of being derived from data wholly unconnected with the place or parties where and by whom the observations on the motion of the glaciers were made, and therefore are free from the remotest suspicion of either in any degree influencing the other.

* This explains a circumstance which has always hitherto been a difficulty to me; the united testimony of the best-informed inhabitants, not only at Chamouni but elsewhere (as at Zermatt and at the Simplon), to the effect that during winter the lowest end of a glacier, which terminates in a valley, does not greatly protrude, nor force the snow before it. This arises in fact from the comparative smallness of the motion which the *tongue* of such a glacier appears to possess, especially in winter.

† Travels, p. 141.

## Table VI.

Mean Temperatures (by periods) on the Centigrade Scale, observed at Geneva and the Great St. Bernard*.

| | Geneva. | | St. Bernard. | | Means of Max. and Min. | |
|---|---|---|---|---|---|---|
| | Max. | Min. | Max. | Min. | Geneva. | St. Bernard. |
| 1844. Oct. 2 to Oct. 14. .. | 17·37 | 8·57 | 4·16 | — 1·58 | 12·97 | 1·29 |
| Oct. 14 to Nov. 2. .. | 11·77 | 5·13 | 1·61 | — 4·93 | 8·45 | — 1·66 |
| Nov. 2 to Nov. 19... | 11·84 | 3·11 | 1·62 | — 5·66 | 7·47 | — 2·02 |
| Nov. 19 to Dec. 4.. | 4·93 | — 0·18 | — 5·11 | —11·25 | 2·27 | — 8·18 |
| Dec. 4 to Jan. 7.... | 1·45 | — 2·40 | — 5·70 | —10·96 | — 0·27 | — 8·33 |
| 1845. Jan. 7 to Feb. 18. .. | 1·47 | — 4·11 | — 6·92 | —13·86 | — 1·32 | —10·39 |
| Feb. 18 to March 18. | 4·76 | — 2·74 | — 3·93 | —12·56 | 1·01 | — 8·24 |
| March 18 to April 17. | 11·27 | 1·57 | 0·19 | —10·13 | 6·42 | — 4·97 |
| April 17 to May 17. | 16·04 | 5·56 | 5·15 | — 6·36 | 10·80 | — 0·60 |
| May 17 to May 31... | 16·83 | 6·91 | 6·04 | — 4·67 | 11·87 | 0·68 |
| May 31 to June 19.. | 23·70 | 12·71 | 11·48 | 1·11 | 18·20 | 6·29 |
| June 19 to July 4... | 22·58 | 12·13 | 9·81 | 0·53 | 17·35 | 5·17 |
| July 4 to July 18. .. | 24·01 | 12·84 | 10·20 | 2·08 | 18·42 | 6·14 |
| July 18 to Aug. 6... | 23·28 | 13·20 | 9·25 | 1·68 | 18·24 | 5·46 |
| Aug. 6 to Oct. 8.... | 21·15 | 11·31 | 7·57 | 0·10 | 16·23 | 3·83 |
| Oct. 8 to Nov. 8.... | 11·84 | 3·78 | 2·38 | — 3·70 | 7·81 | — 0·60 |
| Nov. 8 to Nov. 21... | 12·54 | 5·86 | — 1·03 | — 6·06 | 9·20 | — 3·54 |

A general comparison of the curves of temperature and those of glacier motion (more particularly on the Glacier des Bois) affords a proof of the justness of the principle laid down by me in 1842, that the motion of the ice "is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts†;" the evidence of the connection is plainer by mere inspection than any detail could make it. But I request attention to the apparent anomalies of the curves, as affording a stronger evidence of the fidelity with which the measurements have been made, and to the truth of the plastic theory, than perhaps even the general coincidence just referred to.

If the velocity of the glacier depend upon the completeness of its infiltration with water, rendering the whole an imbibed porous mass like a sponge, it cannot depend solely on the mean temperature of any period, but also upon the *wetness* of the surface, whether derived from mild rain, from thawing snow, or from any other meteorological accident which the register of the thermometer cannot of itself indicate‡. Further, a thick coating of snow on the glacier must defend it from the excessive cold of winter just as it defends the earth and plants, and consequently the minimum of motion will not necessarily coincide with the minimum of temperature. Now to

* The last three lines have been added during printing.

† Fourth Letter on Glaciers, Edinburgh Philosophical Journal, Jan. 1843; and Appendix to Travels, 2nd edit. p. 415.

‡ "The *proportion* of velocity does not follow the *proportion* of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as a great heat would do."—Travels, 2nd edit., p. 372.

estimate these more irregular causes is not so easy; but some light is thrown upon them by a register of the weather and state of the snow, voluntarily kept for me at Chamouni by Auguste Balmat; which forms a valuable supplement to the thermometrical register of Geneva and St. Bernard. Although the daily details would take up too much space, I will endeavour to give a faithful abstract of them so far as to give a general idea of the climate of Chamouni from October 1844 to November 1845. This diary includes (at my request) occasional notes on the state of the source of the Arveiron, which are of considerable interest.

*Weather at Chamouni.*

1844. *October*.—A good deal of rain during the month, which on the 10th and 16th fell as snow on the hills (nine inches at Montanvert), and subsequently to the latter day the glacier at the Montanvert was not clear of snow during the winter. 14th. Source of the Arveiron diminished to *one-fourth* (of the summer volume). Ice-vault more than half closed.

*November*.—Till 14th much rain and snow; fine with frost after. 20th. Source of the Arveiron very low; has not shifted its usual position.

*December*.—Weather generally fine throughout; cold most severe from 7th to 12th.

1845. *January*.—The weather continued splendid till the 20th; greatest cold from $-2°$ to $-5°$ Reaumur. 19th. The vault has disappeared at the source of the Arveiron. 20th. The first snow fell which lay at Chamouni, and continued from this day, attaining a depth of $1\frac{1}{2}$ foot in February. Up to this time all the secondary heights, even the Breven and Flegère, were clear of snow, and the weather suitable for Chamois hunting. Occasional snow till the end of the month.

*February*.—Snow at intervals all the month. 13th. Greatest cold of the season; thermometer $-15°$ Reaum. followed by fine weather. 20th. Snow lies $2\frac{1}{2}$ feet deep at the upper stations on the glaciers; $1\frac{1}{2}$ foot at Chamouni. The arch of the source of the Arveiron has wholly disappeared, but the water issues at the usual places as in summer. The water is reduced to a small amount and may easily be stepped across. It is *still whitish and dirty*, though less so than in summer; *except* when a change of weather is threatened (when it is as dirty as in summer)*. *Same date*. The glacier of Bossons has extended itself much. "On ne s'y reconnait presque plus." It is advancing towards the moraine of 1818; and the lower end is at least seventy feet high.

*March* 1st—3rd, mild, with rain; 3rd—13th, cold; 15th, *heavy rain*. Alternate rain and fine till end of the month. 27th. Not half a foot of snow lying at Chamouni. The source of the Arveiron has not opened a vault. The quantity and muddiness of the water the same as at the last report.

* This important remark proves that in the middle of winter a temporary rise of temperature of the air over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water.

*April.*—First week fine; second week cold with snow; changeable to the end of the month. 16th. Source of Arveiron has not much increased in water since the middle of March. In the end of April the snow first disappeared from the lower part of both glaciers.

*May.*—The first half of the month fine, with occasional snow; the second half changeable, with rain. 17th. The source of the Arveiron has increased three-fourths (*means probably in the ratio of four to one*) since the middle of April, and is dirty. The ice-vault is not yet formed. 26th. The Glacier des Bossons advances rapidly and is crumbling into pyramids. The end of the glacier is at least eighty-five feet high and advances considerably, particularly during the month of May; and widens greatly.

*June.*—A changeable and wet month; a very late season*. The snow did not entirely disappear from the Mer de Glace opposite the Montanvert till the beginning of July. 6th—7th. The vault opened at the source of the Arveiron. The quantity of water since the end of May is the usual summer supply.

*July.*—Commenced with warm weather. 5th. Thermometer 27° Reaum. The snow has disappeared from the ice opposite Montanvert, but some patches remain on the way to the Jardin. The Mer de Glace is much higher in level (about forty feet) than in former years, and the marks made in the rock at the *Angle* (in 1842) are all covered. The crevasses much the same as usual. The glacier of Bossons has also increased greatly, and appears to be approaching its old moraines. The register for the greater part of July has not come to hand.

*August.*—A very changeable rainy month. 8th or 9th. The arch at the source of the Arveiron fell in, and did not form again during the season.

*September.*—Also a changeable month. Rain twelve days.

*October.*—A very fine month. No rain mentioned after the 7th.

A careful examination of this interesting register will explain several of the apparently irregular inflections of the curves of glacier motion. Thus (to continue our general remarks, p. 186) we find

V. At the upper station on the Glacier des Bois the least velocity occurred in December, whilst at the lower station (and at both of those on the Bossons) a minimum coinciding also with that of the temperature of the air took place in January. This coincides with the important fact noted in the preceding register, that the upper part of the Mer de Glace was covered with snow from the 16th of October, which only lay in the valley of Chamouni from the 20th of January; the snow screening the ice from the extremity of the cold.

VI. The comparative march of the two glaciers bears a remarkable relation to their positions and form. In the Bossons we detect at once the sudden transitions and seemingly capricious changes of a torrent; in the Mer de Glace we have the

* It will be seen from the temperature curves that the thermometer *fell* considerably in the latter part of June, both at Geneva and St. Bernard.

stately and regulated flow of a river, in which the slighter variations are absorbed by the predominant inertia of a comparatively stable mass. Now the glacier of Bossons is, as every one who has seen it knows, a mere icy torrent, "a frozen cataract," which descends in a continuous mass from the level of the Grand Plateau of Mont Blanc to that of the Valley of Chamouni with very little impediment, with no confining bulwarks of rock, no contracting straits; and throughout this great vertical height of at least 9000 feet, the angle of descent is very steep indeed for so vast a mass. On the other hand, though the part of the Mer de Glace, called the Glacier des Bois under the Chapeau, is very steep, its "*régime*" is regulated by the supply derived from the reservoir glacier above, and, precisely as in rivers of great magnitude and length of course, and of moderate declivity, it yields sluggishly to impulsive or retarding forces which are checked and opposed by the multitude of sinuosities, the embaying of the ice in rock-bound expansions of the channel, the struggle of its passage through defiles and the enormous friction of its lower surface. Yet, lest we might attribute the irregularities of the torrential glacier to causes quite local and uncertain, we find them reflected more or less distinctly in the movements of the neighbouring one. Thus the anomalous retardation in the end of March and beginning of April appears in three stations out of four, as does that in the first half of June, showing clearly that it is not an error of observation. It appears that the thaw of the winter's snow during the month of May, saturating the pores of the glacier with water, produced (as we know that a thaw always does) a sudden and violent march, especially of the more susceptible or torrential glacier. So completely had this sudden move forced on the glacier of Bossons, encumbered by the spring avalanches and loaded with all the fragments and snow masses which had remained temporarily suspended during the winter months, that the lower part of the glacier (as we read in the memoranda to the register) advanced and widened greatly, to an extent which it had not done for many years past, and seemed to change its whole character; and in February a similar temporary increase of volume had taken place; "on ne s'y reconnait presque plus," writes Balmat; thus accounting for the particular accession of speed which appears in that month. In both cases, after the rapid march in February and in May, a reaction takes place; the material is deficient, the excessive pressure has been removed by the previous overflow, and a lull occurs in March and in June.

VII. These irregularities, such as they are, even should we fail in entirely explaining them, are at least not to be attributed entirely to errors of observation, since different observations (which it is to be recollected were sent to England in so rough a state that they required to be reduced and computed before the variations of velocity could be deduced from them) agree amongst one another, and agree with the phenomena casually noted in the Meteorological Register. They are very trifling in the movement of the Glacier des Bois, which presents a curve of remarkable regularity, giving a minimum about the end of December, and a maximum in July. The coin-

cidence with the curve of temperature is greater throughout than we could have expected, considering the important difference of circumstances which occur in autumn and in spring when the thermometer stands nearly alike, the first chill of autumn depriving the glacier of its fluid pressure more effectually than the severer cold of winter which is tempered by its snowy covering, whilst in spring the first relaxation of the bands of frost saturates the icy mass with the impetuous streams of melted snow, as effectually as the intensest heat of summer. In fact, the velocity would probably be greatest in spring, were it not that then the ice has attained its greatest consolidation by the slow but continued effect of the winter's cold penetrating its upper layers, though after all probably to no very great depth. But this is undoubtedly the reason why the minimum and maximum approach so near to one another in point of time in the torrential glacier of Bossons, and it receives an important illustration from the independent fact of the observed condition of the source of the Arveiron, which (see the Meteorological Register), though very small in February, was still whitish and dirty before a change of weather, showing that the bands of frost were not so strong as to prevent a temporary relaxation of thaw throughout the mass of the glacier even in winter; and although the *mean* temperature of the air had been rising ever since the middle of January, and the greatest cold had occurred early in February, we find that at the end of March the source of the Arveiron was still as small as in February, and that owing to the coldness of the spring it had not even increased very much till the middle of April, when it almost suddenly resumed its summer volume. Now during all this time the velocities of the glaciers underwent but little change,—some oscillations backwards and forwards,—but took no real start until the frost had given way, and the tumultuous course of the Arveiron showed that its veins were again filled with the circulating medium to which the glacier, like the organic frame, owes its moving energy.

VIII. Being curious to see how far a relation might be established between the temperature of the air and the motion of the glacier independent of the irregularly acting causes above adverted to, I projected in Plate XI. fig. 2, the motions of the several points of the glaciers in terms of the temperature of the air for the periods already mentioned. It is to be recollected, however, that the observations of the thermometer were not made on the spot, and indeed it would have been difficult to have fixed upon a spot which should represent the mean circumstances of the whole glacier. Perhaps, therefore, the average of the observations at Geneva and St. Bernard (the mean of whose elevations is 4750 English feet above the sea, and therefore between that of Montanvert and Chamouni) may represent pretty fairly the climateric conditions of the inferior parts of the Glaciers des Bois and Bossons. Now, if we examine the curves of fig. 2, we are struck with *their almost perfect flatness until zero of the centigrade scale of temperature is reached*; but, the thawing point of ice past, the velocity manifestly goes on increasing with the temperature, in a ratio which would appear to be tolerably uniform if we neglect the irregular inflections of the curves.

IX. I am unwilling to multiply deductions which every intelligent reader will draw for himself; but one more I must add. It very clearly appears that the variations of velocity due to season are greatest where the variations of temperature of the air are greatest, as in the lower valleys; but it also appears from Remark VIII., that variations of temperature below 0° centigrade, or 32° FAHRENHEIT, produce almost inappreciable changes in the rate of motion of the ice. Hence, from this circumstance alone, we should deduce that in the higher parts of the glacier (where, for example, it freezes almost every night in summer) the variations of velocity should be least, and indeed comparatively small at different seasons. This is well illustrated by comparing the summer motions of the stations D, A and C, mentioned in the first part of this section, with their annual motion, which exhibit a much slighter excess in favour of the summer period than in the lower stations which we are now discussing. The same thing was observed by M. AGASSIZ's surveyors on the glacier of the Aar, who at first saw, in this not very great inequality, an objection to my theory. On a more searching investigation, however, the objection disappears, as in their later writings they have acknowledged. Their position of observation far up on the glacier of the Aar, in a spot having a mean temperature near the freezing point if not lower, had a summer daily motion of 7·99 inches, and a mean daily motion during the whole year of 6·41 inches*. Now at station C, or the Pierre Platte, on the Mer de Glace, the mean motion for July 1842 was 10 inches, and for the whole year, 1842–43, it was 8·56 inches. It is quite evident that the motion of any point in the midst of a glacier is controlled by that of those which precede and follow it, and that it does not necessarily result, either that all must at once suffer a similar increase or diminution of speed, or that the times of maxima and minima, or even the general form of the annual curve, shall be the same. This leads to an important practical result which we shall follow out in the next section.

### § 8. *Summary of the Evidence adduced in favour of the Plastic or Viscous Theory of Glacier Motion.*

It is often difficult to obtain a calm and full hearing for any new theory or experimental investigation; not because there is any antipathy to novelty, or that experiment is undervalued, but simply because, in an age of bustle and struggle for pre-eminence, each man is so busy with his own reputation, or the means of increasing it, that he has no leisure to attend to the claims of others; to which may, perhaps, be added, that in the general diffusion of knowledge and acquirement, each reader, finding something in every course of experiment or reasoning which he knew, or thinks he knew before, is apt to run off with the chain of ideas which that one familiar link suggests, and losing patience to follow an argument of which he thinks he can, by his own penetration, anticipate the close. He sits in judgment on errors which are of his own invention, and confronts the author with arguments and opinions already

* Comptes Rendus, Dec. 9, 1844.

thrice refuted and rejected by himself. In an age when all men would be teachers and all write for the press, the lot of an attentive reader falls to few.

I am far from saying that I have been more than usually unfortunate in this respect. But having, like others, seen my opinions disfigured for want of sufficient attention to apprehend them, or the arguments by which they are supported; ignorance of first principles hinted at, and even errors of observation imputed, where it was convenient that such ignorance and such errors should be presumed; I claim the privilege of stating afresh, though very briefly, the leading opinions which I do hold, and some arguments for them, which, if not altogether new, may be placed in a new light.

My chief analogies for the illustration of glacier motion have been drawn from the motion of a river, and by that comparison it in a great measure stands or falls. Slight and partial as is our knowledge of the mechanics of imperfect fluids, the explanation which I have given is founded upon that knowledge, and it appears to me to be sufficiently precise to warrant the inference of an identity of the mechanism in the two cases;—namely, that the movement is due to the internal pressures, arising from the weight of the mass, communicated partly or principally in the manner of hydrostatic pressure throughout a body whose parts are capable of moving or being shoved over one another (by that exertion of force which Dr. Thomas Young calls *Detrusive Force**, which overcomes what is commonly called the Friction of Fluids), so that the velocities vary from point to point of the moving body, being most rapid near the surface and centre, and least so near the banks and bottom.

So viscous fluids move, so bodies (even brittle solids, such as hard-boiled pitch) possessing the ordinary properties of solid bodies often do, if sufficient time and sufficient force be allowed†; the efficiency of time being chiefly this, that a pressure insufficient to produce instant detrusion, will, sooner or later, cause the particles to slide insensibly past one another, and to form *new attachments*, so that the change of figure may be produced without positive rupture, which would reduce the solid to a heap of fragments. This change may either take place without any loss of homogeneity, or by numerous partial and minute rents not everywhere communicating, and therefore not necessarily destructive of cohesion, which may be termed a bruise.

*A glacier is not a mass of fragments.*—As the analogy of the glacier to a river, in which the fluid principle is greatly in defect, and the cohering or viscous principle is greatly in excess, is the theory which I maintain, it is evident that the analogy of a stream of sand, or loose materials shot from a cart, or any other comparison with an aggregate of incoherent fragments or individual masses, must be wrong if mine be right. And I feel confident, not only that such an incoherent mass could not move after the manner of a glacier, but also that attentive inspection of a glacier at once contradicts such an idea.

* Lectures, I., 135.

† See Professor Gordon's Experiment, Philosophical Magazine, March 1845.

On the *first* point, I maintain that a rugged channel, like that of a glacier, with a moderate slope, being *packed* with angular solid fragments, would speedily be choked, and that farther pressure from behind (for such a mass can only convey thrusts, not strains) would tend to wedge the fragments more tightly. Some grains of dry sand will slide easily down a plate of glass; but try to thrust it forcibly through a narrowing tube, or even a uniform one, the lower end of which rests on a surface over which the sand has poured, and your effort is vain, the tube will sooner burst; and even rocks may be blasted rather than the power of the wedge yield*. If the figure of the bed or channel be in any degree irregular, that is, have expansions and contractions, however smooth its surface, however small the sliding angle of ice upon that surface, the choking of a strait or contraction by the piling of the fragments will be as complete and effectual as if the lateral friction were excessive. Now in point of fact we have such cases as this;—a glacier 2000 yards wide (the Mer de Glace at the Tacul) issues by an orifice or strait 900 yards wide;—the glacier of Talefre, a nearly oval basin, pours out its annual overcharge by an orifice the breadth of which is but one-third of its lesser, one-sixth of its greater diameter†. On the supposition of jostling fragments, the facility of motion is increased, as the comminution is greater. The impossibility of the discharge of a fragmentary solid through a gorge by long stripes fractured parallel to its length, and constituting parallelopipedons of a certain breadth, is evident.

*Crevasses.*—In the *second* place, I maintain that actual inspection shows that a glacier is not the mass of fragments nor of parallelopipedons which some persons have, naturally enough at first sight, supposed it to be. In truth there is not an approach to such a condition in those glaciers which move over moderate slopes of considerable extent, which have very properly been assumed by all writers as the criterial examples of any theory; for it is not denied that portions of glaciers and glacier tributaries do sometimes fall piecemeal over precipices, each fragment descending by its separate and individual gravity, in the manner of an avalanche, although I am disposed to believe, indeed am sure, that the number of such instances is smaller than is usually imagined; and the angle requisite for such a tumultuous mode of descent is far greater than it has, perhaps, always hitherto been considered to be. To him who would form a just estimate of the mechanical constitution of a glacier—who would consider it as a whole—without *always* distracting his attention from the length and breadth of the problem by a minute attention to its lesser features,—I would earnestly recommend the frequent and attentive survey of a glacier or glaciers from a considerable elevation above their level and under varying effects of light. Had I confined myself to studying crevasses on the surface of the glacier, measuring their depths, injecting the ice with fluids and taking its temperature; useful and important as these inquiries

* See Hubbr-Burnand's conclusive experiments on this subject, Ann. de Chimie et de Physique, xli. 166, and Fechner's Repertorium, i. 65.

† See the Map of the Mer de Glace and its tributaries in my Travels in the Alps of Savoy.

are, (and I might almost include the fundamental and most important inquiry of all, that of ascertaining the velocity of its parts,) I should have been much longer in seizing the general truth of the individual character of a glacier, the importance of the fluid-like connection of its parts, the perfectly secondary importance or unimportance of the fissures by which it is often traversed. The traveller who winds his tortuous and sometimes perilous path amongst these crevasses, forgets, in the fatigue of his circumventions, in the wonder of his curiosity at their beauty and seemingly unfathomable depth, in the appalling steepness of their sides and the comparative insecurity of his own footing—he forgets, I say, in the midst of all these claims upon his attention, his curiosity, and his strength of mind, the comparatively large surfaces of unbroken ice over which he heedlessly walks, and the small, the very small depth at which most of the yawning crevasses which make such an impression on his imagination, dwindle into mere slits;—and when his walk is finished, he imagines that a glacier is a mere network of fissures interlacing in all directions. But let him gain a bold height above its surface, 800 to 1000 feet at least*, so that the whole may be spread somewhat like a map before him, yet not too distant to prevent his seeing the number and forms of the crevasses, and estimating their area compared to that of the unbroken ice, his opinion is first shaken and then changed. He sees in the glacier a *whole*, which, regarded as such, is merely scarred, not dissected by these fissures;—he sees a mass as capable at least of conveying strains as thrusts; of which the cohesion is no more destroyed than (to use a comparison which I long ago employed) a parchment sieve is incapable of being stretched, because it is covered with fine slits.

I am confident that this will be plain to every unprejudiced person who will make the observation which I have recommended, and I have no hesitation in stating my belief that it will be found to be fully confirmed by M. WILD's map of the glacier of the Aar, should it ever be published; I say so without having any recollection how the matter stands, although I once had an opportunity of seeing that fine work for a few minutes; and the verification of this remark, by positive measurement, will, so far as I see, be the chief result likely to flow from the patient and disinterested labour of that competent surveyor.

But if this be true in a merely *superficial* plan, how much more true would it be if we could pare off the upper stratum of the glacier, and view a horizontal section of it at a depth of a hundred feet! The depth of the crevasses has, I am persuaded, been as much exaggerated as the thickness of the ice of the glacier has been underrated. In how few cases (where a glacier does not descend tumultuously) can we

* I may mention, as the very best stations which I am acquainted with, the summit or higher slopes of the hill of Charmoz above Montanvert, Station G*, above Trelaporte, and a point directly above the Couvercle at least 1200 feet higher than the Mer de Glace, which may easily be reached from the glacier of Talefre. Other glaciers offer of course similar points, but few so advantageous; the glacier of the Aar from the Schneebighorn, the lower glacier of Grindelwald from the slopes of the Mettenberg, the glacier of the Rhone from near the Mayenwand, and that of Zermatt from the Riffelberg, are examples.

let a plumb-line down even fifty feet without grazing the sides! and to what an insignificant fissure has the gaping crevasse dwindled even at that small fraction of the glacier's thickness! Supposing the crevasse to become uniformly narrower, how soon would it be extinct!

Again, the crevasses which traverse the surface of the glacier have almost always a determinate direction or directions, of which the simplest type seems to be that of perpendicularity to the veined structure*, which, generally speaking, occasions a convexity of the lines of fissure towards the origin of the glacier. Opposite Montanvert the crevasses form two systems inclined 65° to one another, but this appears to be a casual occurrence arising from a fresh strain being imposed on the ice owing to its rigidity when the direction of the bed or trough suddenly changes, and the two-fold systems probably coexist but for a short space, one tending to close whilst the other opens. Be this as it may, unless where a glacier is falling headlong in the manner of a cascade, the crevasses do not produce any actual dislocation of its mass into blocks or fragments, since the crevasses rarely intersect even where most numerous, but almost invariably *thin out* in the solid mass, whilst another crevasse takes its origin a little to one side or other, leaving a firm connection of ice between them; and the difficulty and danger of traversing a glacier where much crevassed, does not arise from the necessity of leaping from square to square of ice, but from having to traverse these bridges of icy communication, which even there link the glacier together, and which are almost always sharp on their upper edge when the season of the year is pretty far advanced, owing to the continual dripping.

The occurrence of crevasses which cut up a glacier into square or trapezoidal blocks, is sufficiently infrequent to deserve notice. Such occur when a glacier of the second order descends over a boss of granite, or a surface convex in all directions. We have then radiating crevasses combined with concentric ones, producing a tartan-like appearance. Such may be seen in a glacier of the second order on the south side of the Aiguilles of Charmoz and Grepon, above the Glacier du Géant; and it is a very convincing proof of the *essential tenacity* of a glacier, that, with a surface so scarred and intersected, the fragments do not fall away in avalanches. This only is to be explained by the consideration that, thin as are the glaciers of the second order, the apparent dislocation is only superficial.

Were the inequality of the central and lateral movement of the glacier mass to be attributed to longitudinal fissures or discontinuities, by means of which broad stripes of ice slide past each other, we should have to demonstrate the existence of such fissures, which could not be always close unless either (1) the surfaces were mathematically adapted to slide over one another, or (2) the ice possessed sufficient plasticity to mould the surfaces to one another's asperities, in which case the plasticity would alone be sufficient without the discontinuity to explain the motion of the ice. These longitudinal fissures, cutting the common transverse fissures perpendicularly,

* Travels in the Alps, p. 171.

would divide the glacier even where most level into trapezia, and no transverse crevasse could be straight-edged but must be jagged like a saw, or cut *en échelon*. Such a phenomenon never occurs unless where a glacier is moving *torrentially*, or with great disturbance and down a steep. *There* such longitudinal fissures may occasionally be seen, but they form the exception and not the rule. It has been demonstrated by an elaborate proof in § 5, that the only trace of longitudinal discontinuity in the normal condition of the glacier is to be found in the veined structure, which, being caused by a partial discontinuity at a vast number of points, admits of an insensible deformation of the glacial mass without sudden or complete rents, or slips, or the formation of zigzag crevasses.

The existence of the great transverse crevasses, which, even in glaciers not moving torrentially, divide the surface of a glacier by rents perhaps 2000 feet long*, have been thought by some to be comparable to beams of an elastic material, supported at the two ends, and bending under their own weight forward, in the middle. Were this the case, it would scarcely modify the plastic theory as I have propounded it; because in order that such a bar of ice should conform to the known movements of the glacier, opposite the Montanvert for instance, the centre must continually gain upon the sides at the rate of 150 feet per annum at least, consequently the limit of cohesion of an elastic solid would soon be overpassed, and plasticity in the material sufficient to explain the whole motion would inevitably be admitted at last. Independently of this, it is evident, that were such a flexure essential to the motion, the lines of crevasses would be convex in the direction in which the glacier is moving instead of towards its origin.

*Argument from the Equable Progression of Glaciers.*—The equability of the motions of the various parts of a glacier, united as I have shown them to be by intricate relations†, must, I think, appear conclusive to every one capable of forming a just opinion on the subject, that the relative movements of the various parts of the glacier are due to the action of forces at small distances and to the antagonism of molecular cohesions and molecular strains, and not to the casual jumbling of a quantity of rude fragments. To myself, I confess that this now appears the strongest argument of all for considering the glacier as a united mass like a river, in which there is a nice equilibrium between the force of gravitation, acting by hydrostatic pressure, and the molecular resistances of the semi-solid; the degree of regularity of the law which connects the partial movements is wonderful, and I maintain that it is inexplicable except upon the viscous theory. Thus (1) the glacier moves continually, summer and winter, day and night, and never by fits or starts; for if it does—if gravitation overcomes mere friction, it occasions a shock or avalanche; (2) its mean annual motion is nearly alike from year to year; (3) the relative velocities of points widely distributed over the glacier (but exposed to similar influences of climate), change simultaneously in the same directions, often in the same proportions; thus "the variation of velocity

* Travels, p. 171, 2nd ed.

† See § 5 of this paper, pages 167 and 168.

in the breadth of a glacier is proportional to the absolute velocity at the time of the ice under experiment*." (4) The progression of velocity from the side to the centre is marked by insensible gradations†. (5) When we compare the motion of a given point of a glacier any day of one year and the same day of another, the probability is that the velocity will be exactly the same, if the season be equally hot or cold; hence, surely, a most unexpected result, which I first announced in 1842, that *a few days' observation of a glacier will enable any one to compare its mean rate of motion over its various parts and with different glaciers.* Thus, the motion of a point marked D 2 on the Mer de Glace, was in 1842, from August 1 to August 9, 16½ inches daily; from August 9 to September 16, 18 inches; now next year, 1843, *one* observation at the same point in August gave 16 inches; and in 1844, *one* observation in September gave 17½ inches. But still further, (6) the very law of flexure of the ice is the same from year to year: a series of stations across the ice at the Montanvert gave, in 1842, the following (simultaneous) relative velocities‡:—

1·000 1·332 1·356 1·367.

The same points being recovered in 1844, the relative motions were (by a single observation of the space moved over in five days)—

1·000 1·339 1·362 1·374,

ratios almost the same but slightly increasing, which corresponds with the fact mentioned above (3), that when the absolute velocities are greater, the relative velocities are so too, which was here the case, for the velocity denoted by 1·000 was a little greater in the second case than in the first.

*Tensions and Thrusts.*—The occurrence of open crevasses plainly indicates the existence of strains in the ice of glaciers producing disruption, at least partially. Hence some writers have precipitately inferred that the whole glacier must be in a state of tension; an uncertain inference surely in a problem of singular complexity, and one which is not warranted by a more accurate analysis. Yet for a time rival theories seemed poised on the inappropriate question, "Are glaciers in a state of internal tension or compression?" Even if the glacier moved as a mass of fragments, therefore without tension, the cohesion must first have been broken before it could be reduced into fragments. I have been inconsiderately censured for quoting, with approbation§, the observation of M. Elie de Beaumont, that a glacier appears to be rather in a state of distension than compression, whilst I adopted a hydrostatic pressure, acting from the origin as the source of motion. A careful examination of the passages in question will show that my assent to the view of M. Elie de Beaumont was limited to *portions* of the glacier, and especially to those portions most crevassed, the parts, namely, which connect the sides and centre, and which serve to drag the more sluggish, because retarded, lateral portions after the freer central part

* Travels, p. 148.

‡ Travels, 1st edit., p. 146.

† See § 5 of this paper.

§ Travels, 1st edit., pp. 178, 370; 2nd edit., p. 370 and *note*.

on which the *vis a tergo* acts with most advantage; and in a direction generally parallel to the blue bands, so far as they are due to inequalities of motion in the horizontal plane*. My earliest attempts to obtain clear views of the internal forces acting on a semi-rigid body, impelled by self-contained hydrostatic forces, convinced me how little could be founded on the completeness of any mathematical investigation of them, which in our present state of knowledge may well be considered as hopeless; and reserving to myself the not so difficult task of extricating at a future time the more important practical laws of these strains and thrusts, I very carefully avoided, in my first publication, any allusion to what might be considered as their actual distribution; a distribution varying not only from point to point of the glacier surface, but throughout its thickness, and most undoubtedly varying also for the same point at different seasons of the year, or even changing its sign, so that a tension at one season may become a thrust at another.

I had no reason to repent of this caution, from which I only departed so far in my Seventh Letter on Glaciers, published subsequently, as to deduce in an approximate manner, from elementary mechanical laws, the directions of the *surfaces of tearing* within such a mass as I had described, upon the simple supposition that the hydrostatic pressure acting uniformly, the tendency of motion of any particle will be in the direction of least resistance when all the resistances are taken into account, and that the surfaces of rupture will divide particles whose motions are dissimilar, but will not divide particles whose motions are alike. I repeat that I had no reason to repent of my abstinence from theorizing, when I found that a far better mathematician than myself, taking up the inquiry where I had left it, and after applying himself for a long time to the exclusive mechanical considerations which the viscous theory had suggested, left the subject, as I conceive, little more advanced than he had found it, and fell into some mistakes and inconsistencies, almost inseparable from this way of treating a problem which extensive observation and patient thought can alone disentangle.

*Formation of Crevasses.*—It has been seen in the third section of this paper, that De Saussure, and almost all his successors, have regarded the crevasses as *accidents* of glacier motion, and not essential to it; and in this view I of course concur. Nevertheless, the study of crevasses is one of considerable, though secondary interest, and is very far indeed from being completed. It requires, among other things, a very sedulous attention to the state of the glacier at various seasons, and even whilst covered with snow; and it requires further a two-fold classification of crevasses, into those which may be considered as proper to the mass of the glacier, and those which merely seam its surface.

I will first speak of the last point.

Though the formation of a crevasse betokens a local distending force, such a force cannot with any certainty be referred to the whole depth of the glacier below the

* See Philosophical Magazine, May 1845, p. 408.

point where the chasm opens. On the contrary, there is a fully greater probability that under that very spot the ice is compressed. If one cause of a crevasse be, as is universally acknowledged, a protuberance or inequality in the bed over which the ice is impelled, for the same reason that a beam, broken by means of weights, is in a state of longitudinal compression below, where its surface is concave, and of distension above, where its surface is convex, the cracks in the glacier may be due solely to this last and partial cause. Superficial crevasses may consequently be occasioned where there is no *general* distension of the mass, either (1) by the shoving of the semi-rigid glacier as a whole, over a convex declivity, or (2) from an internal turgescence arising from hydrostatic pressure, resisted by the intense friction of the anterior or more advanced parts of the glacier, which, causing the line of least resistance to be upwards and forwards, forces the pasty mass to tumefy or increase in thickness, exactly as it has been seen in § 2, p. 153, that sluggish lava streams do in a similar case. But if the tumefaction be pushed beyond the limits of plasticity of the superior and more distended portions, they must burst and assume the crevassed forms actually observed in the plastic models described in p. 144. Hence the existence of crevasses not only does not always result from a state of *general* distension in the glacier, but may arise from the precisely contrary condition of great internal compression. This argument is well illustrated by the recent observations of M. Agassiz's co-operators on the glacier of the Aar, whose observations I have elsewhere shown* to be incompatible with any other view than that of intense longitudinal compression in the mass generally, and yet the surface abounds in crevasses of the usual form and dimensions.

The manner of formation of crevasses generally, including such as may betoken a real distending force acting on any part of a glacier throughout its thickness, is not only a most curious question in itself, but suggests others which a correct theory of glacier motion can alone answer. If a crevasse once formed remain a fissure in the ice for ever after, why is the horizontal projection or ground plan of the crevasses of a canal-shaped glacier convex towards the origin of the glacier, and not protuberant in the direction of its motion, as the ascertained greater velocity of the centre would assign? Why are the crevasses for the most part vertical and not inclined forwards, or at least not notably so, on the same account? Why, if the glacier be urged downwards by a longitudinal force distending it, do not the crevasses continually widen in proportion as they are further from the origin? These questions seem incapable of a sound answer except by supposing that the crevasses are, at least in a great degree, the fresh production of every spring, and arise from the sudden start which the glacier makes when that extremity which descends into the valley begins to experience the thawing effects of returning summer. I should not wish to speak positively upon what involves a difficult if not impossible observation,—the state of the glacier with respect to crevasses whilst still under the winter's covering of snow. But the fact

* Ninth Letter on Glaciers. Appendix to Travels, 2nd edit., p. 443

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of the transverse direction of the crevasses, or even their convexity towards the origin, from year to year, seems to admit of no other explanation. But besides this, I can affirm, from a careful observation of the crevasses of the Mer de Glace from June to September in one year, that the changes which they underwent were such as preclude the possibility of a crevasse of autumn being merely preserved by the snow of winter, and re-appearing afresh in spring as it had done the previous one. The thing is impossible, because the character of the crevasse is essentially altered. In order that an autumnal crevasse may become a spring crevasse it must be sealed up, annihilated, and opened again. A glance at the three sections in Plate X. fig. 3, will illustrate this. No. 1 shows crevasses freshly opened soon after the snow has quitted the surface of the ice—the edges are sharp, the sides vertical, the openings so small that they may be easily stepped across, and in other instances they are not wider than may admit the blade of a knife. No. 2 shows the crevasse opened to its widest extent by the acceleration of the motion, by the force of the sun which has altogether wasted away the side with the southern exposure, and by the copious drippings of the melting ice and mild rain. No. 3 (which as well as No. 2 is taken from a sketch on the spot, No. 1 being done from recollection) shows what I have elsewhere called the state of collapse of the glacier, which affords the most direct possible evidence of its plastic condition; for we there see, not merely the prominences worn away and blunted by the heat of summer, but subsiding into the hollows, the crevasses being choked by the yielding of their sides, and the glacier again resumes a traversable character, only that the plane surface of spring is changed into irregular undulations preparatory to a complete amalgamation of the whole glacier into one mass*.

The collapse is thus described in my Journal of 1842, written at the time, and therefore more emphatic and unbiassed than after my theoretical views had been matured and published. "1842, Sept. 16, Friday. The level [of the Mer de Glace at the 'angle'] has sunk since the 9th of August, nine feet 8½ inches. The effect of this immense fall is abundantly evident in this part of the glacier. On my first visit this time [*i. e.* after an absence of a month], on the 10th, I was quite struck with its shrunk appearance, as I was today with the collapsed state of the crevasses. There cannot be a question but that the glacier has subsided bodily into its bed, and that the semifused pliancy of its materials causes them to recover a uniform and lower level. The crevasses are much less deep than in July and August, as at that time they were larger and more numerous than in June. They are collapsed and (opposite Trelaporte) almost soldered up; the edges all rounded and melted by the sun's heat." The phenomena here described, "the shrunk appearance," "the *semifused pliancy*," "the soldered crevasses," "the rounded edges," convey to the attentive spectator an intuitive conviction of the plasticity of ice at the thawing season, which no words can

* See Travels, p. 174; and Fourth Letter on Glaciers.

express, no mathematical symbols weave into a demonstration. I can only say that it is easier to believe than to disbelieve; and that sooner or later, it will, I doubt not, be generally admitted.

Considering the crevasses as chiefly superficial in the normal glacier (I mean that of which the inclination is not excessive), it is evident that the formation of the crevasses must depend mainly upon the configuration of the bed. Where the section of the bed parallel to the length of the glacier is convex upwards, there the tension at the surface will cause the crevasses to expand; when the bed is concave and the surface is being compressed, the crevasses tend to close. Hence the surface of the glacier descending an irregular bed may be alternately in a state of distension and compression, and the crevasses do not tend to widen indefinitely, which would be the case if the whole glacier were distended. This tendency in the crevasses to expand and contract in accordance with their position is beautifully seen in viewing the Mer de Glace from a height, as we have recommended. The steep fall opposite Trelaporte shows the expansion of the crevasses, but the comparative level opposite the little glacier of Charmoz gives it time to recover its solidity by the general closing of the crevasses under compression. The careful study of such a scene as this gives a more clear insight into the glacier phenomena than any other part of the inquiry, excepting only the measurement of velocities.

*Law of Velocities.*—To these velocities we now return. The varying velocities in different glaciers, at different seasons and in different parts of the same season, are all in accordance with the motions of a viscous or plastic body. They depend upon the slope, being greatest, *ceteris paribus*, when the slope is greatest; and upon the climate to which the glacier is exposed, being greatest in glaciers which descend into deep valleys, and least in those which, though very steep (such as that of the Schönhorn described in § 6), are placed in so elevated and therefore dry and cold an atmosphere as to afford in sufficient water to moisten the snowy mass or *névé*, and which are therefore endowed with very feebly hydrostatic qualities. This is demonstrated on the one hand by the extreme smallness of their motions, and on the other by the insignificant streams of water to which they give birth even in the height of summer. In any individual glacier the velocity of the parts must (on any theory) vary with the area of section through which the ice stream has to pass; but yet it may happen that the contraction of a valley, if not accompanied (as is often the case) with an increased slope, will oppose so great a resistance to the efflux of the mass, that under intense longitudinal compression its forward motion is retarded, and the condition of uniform discharge is satisfied by the accumulation of the ice in a vertical direction, the rise of the surface being necessarily accompanied with a thrust from below upwards, and a sliding of the particles over one another in that direction. This appears conclusively to be the case for a great extent of the lower part of the glacier of the Aar, as already mentioned, and affords the most direct evidence which

could be desired, that the kind of internal motion necessary for producing the *frontal dip* in the veined structure (which arises from tearing or crushing in sliding in the vertical plane*) was correctly foreseen.

The law of velocities at different points of the axis of a glacier from its origin towards its termination, must evidently depend upon the configuration of each particular glacier. It may be constantly increasing from the origin to the extremity, it may be diminishing, or it may have alternations of increase and diminution; and upon this circumstance the frequency and magnitude of the crevasses will mainly depend. But the *régime* of the glacier, by which we mean to express the combination of circumstances determining its motion, varies from one season of the year to another, owing not only to the general influence of heat and cold, but also to the progressive communication of that influence to portions of the glacier in successive stages of elevation. Evidently the extremity nearest to the valley will receive the earliest and most violent impression of solar heat, whilst the middle and upper regions are involved in complete winter. Partial dilatations must take place in spring, partial condensations in the decline of the year; as is evident from the consideration that temperatures inferior to freezing do not sensibly affect the motion of the ice (see above, p. 192) which higher temperatures do, consequently the influence of season will be chiefly felt in those parts of the glacier where the temperature of the air seldom falls in summer to 32°, whilst the more stable motion of the higher part acts as a drag or equalizer upon the whole system. The condition of violent distension produces crevasses, that of violent compression produces the frontal dip of the veined structure, or that share of it which is due to the relative motions in a vertical plane. The longitudinal veins will result whether the axis of the glacier be distended or compressed. Hence the reason why the frontal dip is difficultly seen in all the middle region of a glacier, which like the Mer de Glace, is subject to much extension due to great and increasing declivity, and to be well seen must be sought for in the higher parts of the glacier, as above Trelaporte, at the foot of the Couvercle†, and in glaciers subject to great compression, as that of La Brenva, the glacier of the Rhone, the Aar, &c.

*Ablation of the Surface.*—One phenomenon is most satisfactorily explained by the variations of velocity established and illustrated in this paper. The collapsed state of the glacier after the hot summer of 1842, and the absolute lowering of its surface level by thirty feet in the space of a few months, had struck me as requiring an energy altogether extraordinary in kind and degree to restore next spring the level which had been lost, in order to allow for an equal ablation the succeeding summer; and at first I was disposed to admit so much of the dilatation theory to be true as would account for the swelling of the surface in a vertical direction by the freezing during winter of the infiltrated water‡. Further reflection convinced me however that this explanation was insufficient and also not required, and I accordingly concluded

* Seventh Letter on Glaciers. Appendix to Travels, p. 435.

† Travels, 2nd edit., p. 167.

‡ Fourth Letter. Travels, App., p. 415.

"that the main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less; the ice more pressed together and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are most wasted*." I have nothing to add to this explanation, except that the observation of the motion throughout the whole year confirms it in every particular. The more elevated portions of the glacier, which during a large portion of the year are exposed to a mean temperature under 32°, move in a manner comparatively uniform, the lower extremities undergo great oscillations in their speed (in the ratio of four or five to one; see page 185); hence the attenuation during the summer *régime* which is owing to the drag taking place downwards in an excessive degree; but the winter's cold, equalizing in some measure the velocity everywhere, brings the plasticity into full action, fills the crevasses and swells the surface to its old level.

As it is universally admitted that the glacier proper does not grow in thickness by snowy accumulations, the important variations in its level in different years† cannot be ascribed to the severity of certain seasons increasing the mass of snow falling upon it, but rather to the prolongation of the winter cold into spring and summer, which causes the condensing or accumulating process to be in excess, and therefore the thickness of the plastic mass to accumulate beyond its due amount.

Thus we have the following phenomena, all independently observed, reconciled and explained by one hypothesis; the general convexity of the crevasses upwards, notwithstanding the excess of motion in the centre; the general verticality of the crevasses, notwithstanding the retardation of the bottom; the perfect state of the crevasses every spring succeeding their visible collapse in autumn; the ascertained velocity of different parts of the glacier, and the diversity of the annual changes which these velocities present; the seemingly opposed facts showing the glacier to be subjected to powerful tension, producing crevasses, and yet to be under a compression which produces in some places the *frontal dip*; and finally, the renewal of the level of the ice during winter, which has been lost partly by superficial melting, but as much or more so by the attenuation and collapse of the glacier during summer. These various effects of one cause, though they do not embrace all the phenomena of glaciers, certainly include a very remarkable and complicated group of facts.

* Travels, p. 386, 2nd edit.

† For instance, it has been seen from Balmat's narrative (p. 189 above), that in 1845 the glacier attained a much higher level at the *Angle* than it had done for three previous years at least, since all the marks of measurements which were cut on the rock in 1842 were concealed; and he attributes this, apparently with reason, to the extreme lateness and coldness of the spring.

*Plasticity—Veined Structure.*—I certainly never expected, when promulgating the viscous theory, that it would have met with so much opposition on the ground that the more familiar properties of ice are opposed to the admission of its plasticity; and that the fragility of hand specimens should be considered as conclusive against the plastic effect of most intense forces acting on the most stupendous scale upon a body placed in circumstances which subject it to a trial, beneath which the most massive constructions of the pyramid-building ages would sway, totter, and crumble. In an age when generalizations of the more obvious kinds are no longer proofs of genius and perspicacity, and when popular writers on science delight to startle their readers by showing how bodies the most dissimilar possess properties in common; in an age in which *gradations* of properties and organs have been studied with such persevering sagacity, and in which so many unexpected qualities have been discovered;—when iron is classed as a combustible, when metals are found which float on water and which catch fire on touching ice, when a pneumatic vacuum is formed and maintained in vessels five miles long, and whose sides are ripped open twenty times a day;—when, moreover, the simpler abstractions of former times are being daily overset, when no body seems to possess any one property in perfection, and all seem to possess imperfectly every quality admitting of degree; when adamant is rejected from our vocabulary, and softness means only less hardness, and the definition of a perfect fluid is as imaginary as that of a solid without weight;—when a vacuum and a plenum are alike scoffed at, and even the heavenly bodies toil through media more or less resisting; when no substance is admitted to expand uniformly by heat, when glass may be considered a conductor of electricity, and metals as imperfect insulators;—in these days, when the barriers of the categories are so completely beaten down, I had not expected to meet with so determined an opposition to the proposition that the stupendous aggregation of freezing water and thawing ice, called a glacier, subjected to the pressure of thousands of vertical feet of its own substance, might not under these circumstances possess a degree of yielding, moulding, self-adapting power, sufficient to admit of slight changes of figure in long periods of time. Still less could I have anticipated that when the plastic changes of form had been measured and compared, and calculated and mapped, and confirmed by independent observers, that we should still have had men of science appealing to the fragility of an icicle as an unanswerable argument! More philosophical surely was the appeal of the Bishop of Annecy from what we already know to what we may one day learn if willing to be taught: "Quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité qui est en opposition directe avec les apparences dont nous venons de parler. *Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats**."

* Théorie des Glaciers de la Savoie, p. 84. Quoted in my Travels, p. 367, 2nd edit. Since this paper was read, Mr. CHRISTIE, Secretary of the Royal Society, has kindly communicated to me a very striking remark upon a well-known and easily-repeated experiment. The experiment is this. If, in the course of a severe

The "ductility" is indeed not great; the compact ice even of the slowest moving glaciers bears evidence, in the veined structure, or "blue bands," to the bruise which it has received from the all-powerful strain which has acted on it. When the difference of motions is excessive, or the slope occasions the speed to be greater than permits the gradual molecular adaptation of the semi-rigid parts to one another, the masses are broken up and fall more or less tumultuously; *the strain being then removed by the dislocation, the veined or bruised structure is invariably extinguished at last.* I shall quote a series of examples of the gradation of phenomena, which I conceive to be plainly connected by a common cause.

1. In any torrential glacier, such as the Glacier des Bossons, the upper part of the glaciers of La Brenva, Allalein, or the Rhone, and many others, the fractures are so numerous that the ice descends in blocks, almost as water in a cascade often does in spray, and hence the internal strains being destroyed, no structure is developed, or if previously developed, tends to wear out*.

2. In a glacier moving torrentially, that is with frequent and considerable changes of velocity, but without being divided into blocks by intersecting crevasses, we find real internal cracks in the ice, some feet in length, and an inch or more in thickness, marked by the pure frozen water which fills these spaces in the comparatively opake whitish ice of which glaciers descending rapidly from the region of the *névé* are composed. Such are peculiarly visible in the lower and more accessible region of the glacier of Bossons†; perhaps the most instructive which can be named as showing these infiltrated cracks, which by their dimensions, direction, and in every other particular, form a true link between the longitudinal dislocations of a torrential glacier, and the perfect veined structure or bruise into which it passes by imperceptible gradations, including a perfectly regular development of the frontal dip, where we might expect it to be well shown, for the observations of page 184 show that the lowest portion of

winter, a hollow iron shell be filled with water and exposed to the frost with the fuze-hole uppermost, a portion of the water expands in freezing, so as to protrude a cylinder of ice from the fuze-hole; but if the experiment be continued, the cylinder continues to grow, inch by inch, in proportion as the central nucleus of water freezes. "In the first instance," says Mr. CHRISTIE, "a shell of ice containing water was formed, no doubt, within the iron shell, and the fuze-hole might be filled by the expansion of the water in the act of freezing; so that there may be no reason for attributing plasticity to the ice as far as this goes; but the shell of ice once formed, and the fuze-hole filled with ice, the subsequent rise of the ice must have proceeded from the ice of the interior shell being squeezed through the narrow orifice. No thawing took place during the process. Does not this show plasticity even in very small masses of ice?" I have also been lately informed, on excellent authority, that in a new work by a most eminent German mineralogist, the plastic character of ice in masses is assumed as an admitted fact. In corroboration of what has been said in the text, I may farther add, that whilst these sheets are passing through the press, I observe in the Athenæum (June 20, 1846), an account of a patent process for moulding solid tin into tubes and other utensils, in the course of which it is stated that "tin under a pressure of about twenty tons to a circular inch, will *run* according to the law of fluids."

* See Third Letter on Glaciers. Travels, Appendix, p. 407.

† See Travels, p. 181.

the glacier of Bossons moves slower than its middle portion; there is therefore a manifest longitudinal compression arising from the friction of the bed*.

3. The next stage is that of the perfect bruise or veined structure, best seen in the most united and least fissured parts of glaciers with rocky sides and moving over a moderate slope. Whatever increases lateral compression (without however necessitating dislocation), such as the union of two or more glaciers in one, tends to develope the structure more perfectly†. Such cases are well seen on several parts of the Mer de Glace, and of the glacier of Miage‡.

4. In very wide glaciers, moving with feeble velocities, the veined structure is slightly developed, except near the sides, simply because the *twist* being small the ice is hardly bruised. Nor can we wonder to find the structure at the distance of many hundred yards from the sides of a vast slow-moving glacier of this description, if developed at all, to be complex and irregular, exhibiting twists such as I have figured in my Travels, p. 164, and which are peculiarly conspicuous in the magnificent glacier of Aletsch. This circumstance finds a precise analogue in the case of a great river, such as the Rhine, or indeed in any river moving with a very slight inclination; the excess of velocity of the central above the lateral parts, not very great at any rate, is distributed over such a space that the slightest casual disturbance of the current, from an irregularity in the bottom or sinuosities of the course, produces local differences of velocity, occasioning ripples and eddies in various parts of the breadth. If these ripples and eddies, in other words, differential motions of adjacent particles, could be visibly represented by using differently coloured fluids, they would undoubtedly afford sections exhibiting undulations and contortions exactly like those which the ice presents in the cases mentioned above. We claim therefore the apparent exception as a real proof of our general rule.

5. In the *névé* proper, no true veined structure is developed; *first*, because, whilst the mass is snowy, its powdery nature yields without admitting of a fracture or bruise; *secondly*, because the true *névé* has rarely any lateral compression worth mentioning, being widely spread and not contained between steep barriers; *thirdly*, because its motion is altogether very small; *lastly*, because its extreme dryness does not afford water enough to percolate its substance and there to be frozen; when it does so, it ceases to be *névé*.

On these grounds I hope that the theory of the veined structure, so important to that of glaciers, may be considered as explaining a number of intimately connected phenomena.

* The internal rents in the lava of Zafarana referred to in § 2 of this paper, and figured in Plate IV. fig. 8, present a perfect analogy with those of the glacier of Bossons, and appear to be due to the same cause.

† Third Letter. Travels, Appendix, p. 407.

‡ See the figures of the structure of the glacier of Miage. Travels, p. 197.

"The glacier struggles between a condition of fluidity and rigidity*." "A glacier is not a mass of solid ice, but a compound of ice and water more or less yielding, according to its state of wetness or infiltration†." "The pressure communicated from one portion to the other, will not be the whole pressure of a vertical column of the material equal in height to the difference of level of the parts of the fluid considered; the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission. * * A glacier is not coherent ice, but a granular compound of ice and water‡." "When the semifluid ice inclines to solidity during a frost, the motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. * * It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevasses§." Such were the terms in which within a few months after suggesting the viscous theory I expressed my opinion of the influence of the compound structure of the glacier, a mass composed, not of ice alone, but of ice including water in its countless capillaries never frozen‖ even in winter. The quality of plasticity or viscosity resulting from the union of a nearly perfect fluid with an imperfect solid is seen in very numerous and familiar instances, as for instance in sand, which is itself devoid of any tenacity until its interstices have been saturated with just so much water as to cause it to flow; or in the still more familiar instance of water-ice prepared for the table, in which the varying proportion of the solid and fluid ingredient gives to it every shade of consistency, from a brittle solid to a liquor including suspended solid grains. The prodigious effect of capillary infiltration in determining the motion of even the most solid and ponderous bodies, breaking up their parts, and giving to the motion of the whole a more or less river-like character, is seen in the frequent case of land-slips, as for instance that of Goldau. And scarcely less instructive are the numerous examples, cited in the first section of this paper, of huge masses of almost cold and brittle lavas being pressed on with a uniform and graduated motion, by the almost unimpeded hydrostatical communication of pressure from the yet active fluid which circulates unseen in their pores. With this analogy before me, I replied in 1844 in the following terms to the question, "How far a glacier is to be regarded as a plastic mass?" "Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semifluid body transmitting pressure horizontally, and whose parts might change their mutual position so that one part should be pushed out whilst another remained behind. But we know in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices

* Third Letter on Glaciers, August 1842. Appendix to Travels, p. 407.

† Travels, 1st edit., 1843, p. 175.

‡ Travels, p. 367., edit. 1843. § Ibid. p. 372. ‖ Ibid. p. 361, 372.

of the former to all depths of the glacier; and as it is matter of ocular demonstration, that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast porous crackling mass of seemingly rigid ice in which it is, as it were, bound up*."

Now the water in the crevices does not constitute the glacier, but only the principal vehicle of the force which acts on it, and the slow irresistible energy with which the icy mass moves onwards from hour to hour with a continuous march, bespeaks of itself the presence of a fluid pressure. But if the ice were not in some degree ductile or plastic, this pressure could never produce any, the least, forward motion of the mass. The pressure in the capillaries of the glacier can only tend to separate one particle from another, and thus produce tensions and compressions, *within the body of the glacier itself*, which yields, owing to its slightly ductile nature, in the direction of least resistance, retaining its continuity, or recovering it by re-attachment after its parts have suffered a bruise, according to the violence of the action to which it has been exposed.

The action of warm weather in accelerating the movement of the glacier is plainly due to the abundance of the water saturating its pores; but this may act in two ways; first, by rendering the frame-work of ice less brittle when it is in the very act of dissolving by the circulation of water in a perfectly fluid state through its pores†, and secondly, and more particularly, from the hydrostatic effect of *gorging* a porous mass with fluid. When an incipient frost dries even momentarily the surface of the glacier, the vast porous mass begins to *drain*. This is a very slow process, owing to the resistance to the passage of a fluid through very long and complicated canals. Were it not so, glaciers would be entirely dry after sunset and in winter, which is not the case. The hydrostatic pressure within the whole glacier is however sensibly diminished by the process of drainage; this is evident from watching the level of water in a vertical hole of any depth made within the solid ice of the glacier. After much rain or heat this level is always higher than after dry cold. In the former case the glacier may be said to be gorged, the supply of water from the surface exceeding the power of the drainage to carry it off. The circulating vessels are therefore overcharged. In the latter case the superficial supply is stopped, the drainage goes on slowly though uninterruptedly, and the level of the water in the vertical shaft slowly descends, indicating the diminution of internal pressure. If it were not for the capillarity of the ducts, it is plain that no effective hydrostatic pressure would

* Sixth Letter. Appendix to Travels, 2nd edit., p. 428.

† This I think is undeniable, from the appearance of the collapsed crevasses above referred to, notwithstanding the difficulty of imagining any variation in the sensible heat of water circulating in ice. It is not the only fact in the glacier theory which seems to require some modification of the commonly received laws of latent heat at the very limit of congelation and liquefaction.

be developed at all; the flow being equal to the supply, no part of the *vis viva* would be expended in producing internal pressures. With this concluding observation I commit the Plastic or Viscous Theory of Glaciers to the impartial judgment of those qualified to decide on its merits in explaining facts, and on the variety of difficult and complicated considerations which opposed and still oppose themselves to a complete development of it.

*Edinburgh, Jan.* 10, 1845.

## XV. *Supplement to a Paper "On the Nervous Ganglia of the Uterus."*

*By* Robert Lee, *M.D., F.R.S., Fellow of the Royal College of Physicians, London.*

Received June 14,—Read June 19, 1845.

In the First Part of the Philosophical Transactions for 1841, I have described and represented in two engravings the nervous ganglia, situated on the sides of the neck of the uterus, in which the great sympathetic and third sacral nerves unite, and from which branches proceed to the vagina, bladder, rectum, and the whole of the lower part of the uterus. In an Appendix to that paper, published in the Second Part of the Philosophical Transactions for 1842, there is contained a further account of the nervous structures situated on the fundus and body of the uterus, and an engraving in which the appearances they present at the full period of gestation have been accurately delineated. From the form, colour, vascularity, and general distribution of these structures, and from their branches actually coalescing, and being continuous with those of the great sympathetic and spinal nerves, I inferred that they were true nervous ganglionic plexuses, and formed the nervous system of the uterus enlarged during pregnancy.

In a gravid uterus at the full period I have recently, and with still more care, traced the great sympathetic and spinal nerves into the two hypogastric ganglia, and from thence over both sides of the uterus to the fundus. A lens which magnified six diameters was employed in this dissection, which enabled me with unerring certainty to distinguish and to separate the nervous filaments from the fine cellular membrane by which they are so closely surrounded, and from all the other contiguous structures. In this minute dissection, many of the details of the nervous system of the uterus are more perfectly shown than in any previous dissection made by me, and they confirm, in the most complete manner, the accuracy of all that is contained in my previous communications on this subject to the Royal Society. To this preparation I can now appeal, as affording a perfect demonstration of the truth of all my statements respecting the ganglia and other nervous structures of the uterus.

## XVI. *On the Nerves of the Uterus.* *By* Thomas Snow Beck, *Esq.*, *Surgeon.* *Communicated by Sir* Benjamin C. Brodie, *Bart.*, *F.R.S.*

Received May 29,—Read June 19, 1845.

In February 1843, during the examination of the body of a woman who died from uterine hæmorrhage immediately after delivery, it occurred to me that the uterus afforded a good opportunity for examining the condition of the nerves in the impregnated state, to which attention had been directed by Dr. Robert Lee in communications to this Society in the years 1841 and 1842. With the view of confirming the researches of Dr. Lee, I commenced the dissection of the nerves of the gravid uterus, but found so many points at variance with his published statements*, and the nerves so small, considering the size of the organ, that it appeared very doubtful whether or not they had increased in size during pregnancy; but in order to arrive at a correct conclusion on this point, a comparison of the nerves of the unimpregnated with those of the gravid uterus was indispensable; and in the following year I commenced a dissection of the nerves of the unimpregnated uterus and of the neighbouring organs.

* To the structures described as nervous ganglia and plexuses, Dr. Lee gave the following names: "The posterior subperitoneal ganglion and plexuses," "a great nervous web under the peritoneum," to which "it adheres firmly," and which covers the posterior surface of the uterus "as high as the fundus." "It represents the appearance of a layer of dense structure composed of fibres strongly interlaced together, having a yellowish-brown colour." "The anterior subperitoneal ganglia and plexuses," "a nervous and vascular mass of great extent, and similar in structure to the subperitoneal ganglia on the posterior surface." It "adheres to the peritoneum firmly," and covers "the whole anterior surface of the uterus." "The left subperitoneal plexuses." "Another ganglion of considerable size," "near the edge of the uterus." "The great transverse plexuses," "which proceed across the uterus," and "present the appearance of a white, pearly fuciculated membrane about a quarter of an inch in breadth." "The left spermatic ganglion," "a dense reddish-brown coloured mass consisting of fibres firmly interlaced together." "The hypogastric or utero-cervical ganglion," which "exceeds in size the semilunar ganglia of the great sympathetic," and "is nearly two inches in breadth." "The middle vesical ganglion," and "the internal and external vesical ganglia," which "form a great web of ganglia and nerves on the side of the vagina." Dr. Lee further adds, "It is chiefly by the influence of these nerves, that the uterus performs the varied functions of menstruation, conception, and parturition, and it is solely by their means that the whole fabric of the nervous system sympathises with the different morbid affections of the uterus. If the nerves of the uterus could not be demonstrated, its physiology and pathology would be completely inexplicable." In the progress of my dissections, I found that the structure considered by Dr. Lee as "the hypogastric or utero-cervical ganglion" was a mass of dense fibro-cellular tissue enveloping several small ganglia and a nervous plexus, formed at the junction of the lateral hypogastric plexus with branches from the sacral nerves; that the "vesical ganglia" were also a mass of fibro-cellular tissue, in about the centre of which were situated small ganglia; and that the remaining structures described as ganglia and plexuses were not nervous structures.—(May 1846.)

The gravid uterus I had dissected was that of a woman about thirty-five years of age, and of middle stature; and to obviate as much as possible any objection that might be raised on the ground of individual peculiarity, the organ selected for the second dissection was that of a woman about the same age and stature as the former. She had also borne children.

The comparison thus made demonstrated the truth of the former supposition, and clearly showed that the nerves had undergone no change of size during pregnancy, nor of position, except such as resulted from the enlargement of the organ over which they were distributed.

*The Great Splanchnic Nerve and Semilunar Ganglion.*—The great splanchnic nerve is said to be formed by branches which come from the sixth, seventh, eighth and ninth, and sometimes tenth, thoracic ganglia. These branches, after passing forwards and downwards, unite at the side of the dorsal vertebræ, and constitute the nerve, which after piercing the diaphragm ends in the semilunar ganglion. If the branches said to come from these ganglia be examined, it will be seen that this is only their apparent origin, and that they are branches of the intercostal nerves. The branches of communication between the spinal nerves and the thoracic ganglia consist of two distinct portions, marked by difference in colour, consistency and distribution. One portion, the smaller of the two, is soft, semitransparent, and of a light brown colour. It leaves the posterior border of the thoracic ganglion, and joins the spinal nerve about one-eighth of an inch anterior to the giving off of the white portion; a gray communicating cord also passes between the thoracic ganglia. This ganglionic mass, when first exposed in the recent dissection, was of a deep red-brown colour, which quickly faded to a light brown by the action of the water in which it was examined. The other portion, about one-half larger than the former, is firm, of an opake pearly-white colour; it passes to the anterior surface of the ganglion, and divides into two portions. One of these portions joins with a similar part, sent down from the ganglion next above, and then goes out in a curving direction on the side of the dorsal vertebræ to contribute in the formation of the splanchnic nerve. The other half of this branch of the intercostal nerve turns down, constituting a part of the intercommunicating cord between the ganglia, to the ganglion next below, and there joining with a similar portion from the intercostal nerve on a level with this ganglion, passes forwards to join the other branches which form the splanchnic nerve. Each branch sent to the splanchnic nerve contains branches from at least two intercostal nerves, viz. from the nerve on the level with its apparent origin, and from the nerve next above it. This arrangement of the nervous fibres is very apparent in the recent dissection, from the marked distinction which exists in colour and consistency between the gray and white portions of the so-called trunk of the sympathetic.

The splanchnic nerve formed by the branches of the intercostal nerves is a single round cord, which increases in size on the addition of each branch. About an inch below the junction of the last branch it separates into two parts, and upon one of these

a ganglion is formed, similar in appearance to those found on the posterior branches of the spinal nerves. About an inch below this ganglion, the two portions of the splanchnic nerve interchange some fibres which form a small plexus. This plexus terminates in several funiculi, which, surrounded by a common cellular envelope, pierce the diaphragm, and end in a collection of ganglia at the side of the cœliac axis.

This collection of ganglia, when considered collectively, is named the semilunar ganglion, and is described as consisting of six or eight ganglia with cords connecting them together. Yet each of these six or eight ganglia is an aggregation of smaller ganglia. The connecting cords sometimes contain ganglionic corpuscles, but most frequently they are composed of only nervous fibres. Six or seven of the funiculi of the splanchnic nerve end in a common ganglion at the superior and outer corner, and the remaining three pass behind, and, joining with a branch from the lesser splanchnic, end in a ganglion placed anterior to the root of the renal artery. From this collection of cœliac ganglia branches are sent to the viscera in the neighbourhood*.

* Professor Cruveilhier, in his Anatomie Déscriptive, vol. iv. p. 1011, says, "Si, sur une pièce qui a macéré dans l'acide nitrique étendu, on cherche à déterminer d'une manière exacte le point le plus élevé de l'origine du grand nerf splanchnique, on verra, d'une part, que les filamens blancs dont la réunion constitue ce nerf, sont déjà distincts au niveau du troisième ganglion thoracique, et, d'une autre part, que ces filamens blancs sont simplement accolés au cordon de communication ainsi qu'aux ganglions, qu'ils se continuent avec les branches émanées des paires rachidiennes; en sorte que l'anatomie de texture démontre de la manière la plus évidente, la continuité du nerf splanchnique avec les paires rachidiennes."

The branches of the spinal nerves which form the white connecting cords with the sympathetic, and which chiefly form the great splanchnic nerve, can be readily traced as high as the third thoracic ganglion. And if the sympathetic be examined in dilute acetic acid, or a solution of bay-salt and arsenite of soda, smaller branches which progressively decrease as we ascend, may be traced from all the superior thoracic ganglia. The branches which form the splanchnic nerve are not exclusively derived from the intervertebral costal nerves, but are partly composed of gelatinous nervous fibres derived from the thoracic sympathetic. These gelatinous fibres, although associated with the tubular fibres of the spinal nerves, are yet distinct, and so connected with the thoracic ganglia above and the semilunar ganglion below, as to leave no doubt in my mind that they exist as an independent system of nerves. The gray roots of the sympathetic, as they are called, are branches of the sympathetic given off from the thoracic ganglia, in the same manner as the branches of the sympathetic are given off from the semilunar ganglion. Its composition, as shown by the microscope, is also analogous to that of the branches of the sympathetic nerve, consisting chiefly of gelatinous nervous fibres, with some tubular fibres. It arises from the thoracic ganglion by a single trunk, soon divides, and is chiefly distributed to the vessels: some of the gelatinous fibres join the intercostal nerve, and, becoming associated with other gelatinous fibres which arise from the ganglion on the sensitive root of the spinal nerve, go with the intercostal nerve in its distribution. The ganglia on the sensitive root of the spinal nerves become thus associated with the ganglia of the sympathetic, whose office, anatomically considered, is to give origin to the gelatinous fibre. A few gelatinous fibres are found in both the motor and sensitive roots of the spinal nerves, but the quantity is so small when compared with the quantity found in other parts, as to preclude the idea that the sympathetic arises from them. Most probably these fibres are distributed to the vessels of the cord; as gelatinous fibres, which come from the gray branch of the thoracic ganglion, pass along the motor root in the direction of the spinal cord, and gelatinous fibres, from the same source, also enter the ganglion on the sensitive root; both these sets of fibres evidently pass from the thoracic ganglion towards the spinal cord. We have thus two distinct and separate

*The Lesser Splanchnic Nerves.*—These nerves having suffered some injury in the removal of the preparation from the body, I cannot decidedly give their origin; but they appear to be derived from the intercostal nerves in the same manner as the greater splanchnic. They are two in number; the superior divides into two branches, one of which joins the ganglion at the root of the renal artery; the other branch divides into three filaments, of which one ends in a ganglion at the side of the aorta, whilst the remaining two end in ganglia on the renal plexus. The inferior splanchnic nerve (posterior renal of Walter) is chiefly distributed to the renal plexus, but sends one branch to the ganglion at the commencement of the aortic plexus.

*The Renal Plexus.*—The kidney is supplied by about fifteen branches, twelve of which proceed from the outer margin of the semilunar ganglion, and three branches from the ganglion in which the lesser splanchnic nerve ends. These nerves in their course assume a plexiform arrangement, and form several (eight or nine) smaller ganglia varying from the $\frac{1}{10}$th to $\frac{1}{40}$th of an inch in diameter, which appear of the same kind as those previously noticed by Remak in various parts of the sympathetic. Some of the branches from the lesser splanchnic nerves also end in this plexus.

*The Nerves of the Supra Renal Capsule.*—From the centre of the semilunar ganglion, three branches pass upwards and outwards to this body. Three other small cords are also supplied by the ganglion in which the principal part of the greater splanchnic nerve ends.

*The Superior Aortic Plexus.*—This plexus (plexus intermesaraicus vel plexus aorticus abdominalis) is situated in front of the aorta, between the cœliac axis and the inferior mesenteric artery. It is formed by four or five branches which proceed from the ganglion before noticed at the root of the renal artery; by two or three branches from the ganglion at the side of the aorta in which part of the lesser splanchnic nerve terminates; and by branches from the superior lumbar ganglia. These branches passing downwards and inwards unite with similar branches from the opposite side, and form a plexus in front of the aorta. Several smaller ganglia are formed in the plexus, some of which are so minute as to measure only $\frac{1}{75}$th of an inch. Minute branches are sent from this plexus towards the aorta, and are lost in its cellular coat,

systems of nerves; one composed of gelatinous nervous fibres which have their origin at the different ganglia; the other composed of tubular nervous fibres which arise at the brain and spinal cord; for notwithstanding the opposite opinion held by Volkmann and Bidder, founded as it is upon very careful and elaborate investigations, I have assured myself, that the tubular fibres, which are mixed with the gelatinous in the gray cords of the sympathetic, are really derived from the cerebro-spinal nerves, and do not originate in the ganglia. The claim of the gelatinous fibres to be considered true nervous fibres is further shown, by many small nerves being entirely composed of them, and by the ultimate distribution of many of them to the arteries. The so-called white roots of the sympathetic are then branches of the spinal nerves, and are distributed to the viscera; and the so-called gray roots, branches of the sympathetic, which passed from the thoracic ganglia, and are distributed to the vessels in the neighbourhood. These facts were demonstrated by preparations at the time this paper was presented to the Society, but in consequence of their differing from the received opinions upon the subject, I was diffident of having drawings made from the preparations. The drawings, with their accompanying explanations, will be presented in a supplementary paper.—(May 1846.)

but being so fine it was impossible to trace them further, or to preserve them in the dissection. They measure $\frac{1}{500}$th of an inch in diameter.

*The Ganglia at the Root of the Inferior Mesenteric Artery.*—These ganglia, after receiving the terminal branches from the superior aortic plexus and branches from the three lumbar ganglia, furnish the nerves accompanying the inferior mesenteric and superior hæmorrhoidal arteries, and send from their lower border branches which form the inferior aortic plexus. The branches supplied by the lumbar ganglia are branches of the spinal nerves, derived in the same manner as those constituting the greater splanchnic nerve. These branches pass over the lumbar ganglia, and becoming associated with gelatinous fibres, end in the ganglia at the root of the inferior mesenteric artery; in the same way as the branches passing over the thoracic ganglia, form the splanchnic nerve, and end in the semilunar ganglion.

*The Inferior Aortic Plexus.*—The plexus, which forms a continuous plexiform arrangement extending from the root of the inferior mesenteric artery to its division into the lateral hypogastric plexus, below the bifurcation of the aorta, has been variously named: it has been called by Valentin *Plexus divisionis aortæ superior et inferior*; and the portion below the bifurcation of the aorta has been called by Tiedemann *Plexus hypogastricus magnus superior, vel uterinus communis*. It is formed by twelve or fourteen branches, which proceed from the inferior borders of the ganglia at the root of the inferior mesenteric artery, and passing inwards, unite with similar branches from the opposite side, and form a plexus in front of the aorta. The plexus has no distinct ganglia in its course, nor has it any marked division, but at the bifurcation of the aorta six or seven filaments are sent along the common iliac artery. Some of these accompany the internal iliac artery, and join the pelvic plexus at the side of the vagina, whilst one branch joins the third sacral nerve midway between the third sacral foramen and the sacral plexus. The remainder of the nerves which pass along the common iliac artery from the aortic plexus accompany the external iliac artery.

The inferior aortic plexus, an inch and a half below the bifurcation of the aorta, divides into the two lateral hypogastric plexuses, where a few crossing fibres occur. These crossing fibres are surrounded in this exposed situation by a dense cellulo-fibrous covering, forming a mass which has been figured and described as a triangular ganglion of a dense and firm structure. Only a small ganglion however exists in this situation.

*Lateral Hypogastric Plexus.*—It is difficult to ascertain what portion of this plexus Professor Tiedemann understood by the plexus hypogastricus lateralis, vel uterinus superior, but probably it was the middle portion, where he has figured it joined by the branches from the sacral ganglia. The plexus extends from the division of the inferior aortic plexus, to the junction of the branches from the sacral nerves at the side of the vagina, and forms a lengthened plexus, situated on the side of the pelvis. Like the inferior aortic plexus it forms no large ganglia in its course; but the nerves

which compose it several times unite and again separate, and at each point of junction a distinct swelling or knot is perceived, which contains ganglionic corpuscles lying by the side of the interlaced nervous fibres, though in too small a quantity to produce the usual red appearance of ganglia. A few branches of communication pass between this plexus and the branches accompanying the superior hæmorrhoidal artery.

*The Pelvic Plexus.*—The union of the branches from the lower part of the lateral hypogastric plexus, with branches from the sacral plexus, constitutes a plexus so peculiar and distinct that I have ventured to propose the name *Pelvic Plexus* for it. It furnishes branches of supply to the bladder, vagina, and rectum, and corresponds with the plexus hypogastricus lateralis, vel plexus uterinus inferior, vel plexus gangliosus, of TIEDEMANN. Twelve or thirteen branches are given off from the third sacral nerve, five or six from the fourth, and one from the second, which passing to the side of the vagina join the short branches which descend from the lateral hypogastric, and the branches which descend with the superior hæmorrhoidal artery, and constitute a plexus. At the points of union between the branches from the sacral nerves, and the branches from the hypogastric, small ganglia generally exist. Several smaller secondary ganglia are also formed in the plexus. The ganglia in this situation are much smaller than those at the root of the superior hæmorrhoidal artery, and the nerves which go to the bladder and vagina contain a much larger amount of tubular nervous fibres than those which proceed to the uterus, the rectum, the intestines, &c.

*The Nerves of the Bladder.*—From the anterior part of the pelvic plexus, branches of considerable size are sent to the bladder, which at first pass amongst the vessels of this part, but do not continue with them in their distribution on the organ. A few minute ganglia are found in their course.

*The Nerves of the Vaginal Erectile Tissue.*—From the lower part of the pelvic plexus ten or twelve branches descend to the erectile tissue, situated at the lower and anterior part of the vagina. These nerves are joined by five or six branches, which come directly from the third and fourth sacral nerves, and at the junction of the branches from these two sources, small yet distinct ganglia, seven or eight in number, are formed, from which nerves are sent to the erectile tissue. The branches supplied by the sacral nerves course along the posterior border of the pelvic plexus, but do not communicate with it.

*The Hæmorrhoidal Nerves.*—The rectum is supplied by minute filaments which come from the posterior border of the pelvic plexus, and accompany for the most part the branches of the hæmorrhoidal artery. They, however, are not the direct continuation of those nerves which accompany the superior hæmorrhoidal artery, for these end in the posterior border of the pelvic plexus.

*The analogy which exists between the different collections of Ganglia.*—From the foregoing facts it will appear that a great analogy exists between the collections of ganglia at the cœliac axis, at the superior hæmorrhoidal artery, and at the pelvic plexus. The same elements exist in each, viz. tubular nervous fibres derived from

the spinal cord, and gelatinous nervous fibres derived from the ganglia; the size of the ganglia being in apparent relation to the quantity of gelatinous fibres required for the nerves distributed from these points. The constitution of the nerves which come from these ganglia differs according to the size of the ganglia. Those distributed to the bladder and vagina differ from the nerves sent to the uterus, intestines, &c., in containing a much larger amount of tubular nervous fibre*.

*Nerves of the Unimpregnated Uterus.*—The nerves which compose the hypogastric plexus, on approaching the neck of the uterus, begin to separate, having larger spaces between them, and on a level with the os uteri they are joined by the branches already described, which accompany the superior hæmorrhoidal artery. This expansion appears to be only the spreading out of the posterior portion of the hypogastric and not of the anterior portion; for the latter, after being joined by the branches which accompany the iliac arteries, passes directly onwards by the broad ligament and supplies the lower half of the uterus. These nerves, or it may be said these continuations from the anterior part of the lateral hypogastric plexus, pass towards the uterus, and mixing with the arteries of this part, go on in company with them for some part of their course, but as they approach the body of the organ they separate, and each pursues a separate distribution. As the nerves approach the uterus they lose the plexiform character, and form a number of delicate branches, which pass on as distinct fine cords, dividing and subdividing, but not uniting with each other. For the supply of the middle part of the uterus a distinct branch comes from the inferior aortic plexus, and running down by the side of the pelvis, receives no communication from the hypogastric, but passes directly to the superior part of the body of the uterus. It there divides, supplying the portion between the insertion of the Fallopian tubes and the termination of the previous branches. It also sends a branch to the ovary. The fundus of the uterus, or the portion above the Fallopian tubes, is sometimes supplied by a branch from the nerves furnished to the ovaries, as seen in the dissection of the gravid uterus.

Distinct from these nerves is another set, which comes from the same continuation of the hypogastric plexus, but assumes a plexiform arrangement round the vessels, and has the distinctive character of forming minute ganglia here and there. These nerves are so minute, that a special dissection is required to determine their ultimate distribution, although their arrangement would lead to the inference that

* The sole anatomical purpose of the ganglia is apparently to give origin to the gelatinous fibres, to which end they are distributed irregularly over the body, in those situations where a supply of the gelatinous fibre is required. The true sympathetic system appears to be a system of gelatinous nervous fibres, which are distributed everywhere over the body, and which preside over the organic functions. It appears to exist independently of the brain or spinal cord, although the tubular fibres which come from the brain and spinal cord are associated with the gelatinous fibres in the larger branches of the sympathetic. In the minute branches the gelatinous fibres exist, separate and distinct from the tubular, and are distributed to the arteries. They may however have other distributions; but this, together with the ultimate distribution of the tubular fibre, remains for future inquiry.—(May 1846.)

they were especially destined for the vessels. Before the nerves were cleared from their neurilema, I frequently met with what appeared to be nervous branches distributed to the arteries, but in every instance I have been able, by the aid of the microscope, satisfactorily to ascertain that these were minute vessels passing from the artery to the nerve to ramify in its neurilema, and not branches of the nerve going to the artery. This was proved by their elasticity, by the absence of transverse striæ, and by the mode of distribution; for they ended abruptly at the artery by a single large termination, whilst they divided and branched to the nerve. It was proved, moreover, by their presenting when divided the opening of a distinct yet small tube, from which air escaped when blown into the artery. I have been unsuccessful in tracing large branches of nerve to the ureter, though I have carefully examined many which in the first instance appeared to be nerves; but all of which proved to be small vessels or rounded cords of fibrous tissue, with the exception of one small filament, which came from the anterior margin of the hypogastric plexus after its junction with the branches which accompany the internal iliac artery, and passing upwards to the ureter, divided and apparently supplied it. From the delicacy of this filament and its position in the dissection it was impossible to preserve it.

The Fallopian tube is supplied by a branch which passes off from the anterior border of the hypogastric plexus a little above the level of the os uteri, and, entering the broad ligament, receives two branches from the nerves which accompany the internal iliac artery, and branches from the nerves between the bladder and uterus. It then passes up to supply the Fallopian tube. The branches derived from the nerves between the bladder and uterus were accidentally divided in an early part of the dissection, hence I am unable to speak decidedly from what source they came. That they were branches of augmentation and not of division, is shown by the trunk of the nerve being enlarged after each junction.

*The Vessels of the Uterus.*—When the vessels of the uterus are divided transversely, they present a wavy outline at the inner border of the section, and when opened longitudinally, the inner surface presents a number of folds, resembling the rugæ of the internal surface of the stomach or bladder when contracted. From these appearances, in which the internal and middle coats of the artery are involved, I am inclined to think that the vessels do not diminish in size after parturition, but are only contracted in their cavity, ready to be again stretched out upon a larger quantity of blood being sent to the organ. This point however remains for further examination, as also the change which occurs during the first pregnancy, in which we would, *à priori*, suppose that the vessels do increase in size.

*The Nerves of the Gravid Uterus.*—After the description of the nerves of the unimpregnated uterus, it is only necessary to mention that the nerves of the gravid uterus follow the same course and distribution in all material respects. They however differ in two slight points. 1st. In consequence of the increased size of the parts, the pelvic plexus and the inferior part of the hypogastric plexus are extended

over a larger surface, which allows their arrangement to be more readily determined. The continuation of the hypogastric plexus, which supplies the uterus, is seen to approach the side of that organ in company with the vessels, and afterwards to leave them, some of the branches passing to the anterior, and some to the posterior surface, both of which finally enter into its substance. 2ndly. The course taken by the nerve which supplies the middle portion of the uterus appears to be different in the two dissections. This however is more apparent than real, for the nerve passes off from the hypogastric plexus high up in the pelvis, and coming down in company with it interchanges some filaments. On reaching the neck of the uterus it enters the organ, and passing up in a distinct tendinous canal immediately beneath the surface, neither receives nor gives any branches until it reaches the portion a little below the insertion of the Fallopian tubes; there the tendinous canal ceases, and the nerve divides into two branches, one supplying the anterior, and the other the posterior surface. The anterior branch, the larger of the two, after entering deeply into the substance of the uterus, again approaches the surface at the insertion of the Fallopian tube, and apparently sends some filaments to it; but whether this is the case or not I am unable to decide, as the nerve was accidentally divided.

*The Nerves of the Ovary.*—The nerve supplying the ovary passes down from the renal plexus in company with the spermatic artery, and when about two and a half inches from the gland it separates from the artery, taking a slightly different course to reach the middle of this body. It there divides into four branches, of which three supply the ovary, and the fourth, passing onwards to the uterus, again divides into two branches, which enter the substance of the uterus on the anterior and posterior aspect of the fundus. Before the nerve reaches the ovary to divide into its terminal branches, it forms a considerable fusiform enlargement.

*The Size of the Nerves.*—The size of the nerves in both these dissections is essentially the same, and when the nerves are carefully compared, no doubt is left that the nerves of the gravid uterus have undergone no change in size; nor any change in position, except that consequent upon the development of the organ. Yet at first sight the nerves of the unimpregnated uterus appear even larger than those of the gravid uterus; this however is due to the manner in which they are arranged. In the unimpregnated state of the uterus, the nerves of the lower portion of the hypogastric and pelvic plexuses, are crowded and packed together so as to occupy a much smaller space than they do in the gravid state of the uterus. The consequence of this is, that the nerves of the former present a wavy arrangement, which gives them the appearance of greater thickness and plumpness. The close manner in which they are packed, and the wavy aspect they assume, can only be fully appreciated when they are first exposed; for in the after process of cleaning and isolating them from the surrounding tissues, these characters are considerably altered.

We may therefore conclude—

1. That the uterus receives its nerves from the hypogastric plexus. It ought

however to be mentioned that some have considered, on physiological grounds, that small branches derived from the sacral nerves, after their union with the pelvic plexus, might reach the neck of the uterus by a circuitous route; but this appears contrary to anatomical evidence.

2. That the ovaries are supplied by branches derived from the renal or aortic plexuses.

3. That the Fallopian tubes are supplied from the hypogastric and aortic plexuses, though it is probable they also receive some branches from the nerves at the superior part of the bladder.

4. That the bladder and vagina are supplied by branches from the pelvic plexus, which contain a large amount of tubular nervous fibres derived from the sacral nerves.

5. That the rectum also receives its nerves from the pelvic plexus, but they on the contrary contain a small amount of tubular fibres.

6. That the nerves undergo no change during a second pregnancy.

7. That it is probable the vessels do not decrease in size after parturition, but are only contracted in their cavity.

Important however as these conclusions are, there yet remain several points which require further investigation; for instance,—

1. Whether any and what change takes place in the nerves at the first pregnancy*. For although it is clear no material difference exists between the nerves of the two preparations described, yet it must not be forgotten that both women had previously borne children.

2. Whether any and what change takes place at puberty.

3. The condition of the vessels in a first conception and their subsequent state.

4. In what consists the great enlargement of size in the gravid uterus.

5. Is there any difference in the composition of the nerves distributed to the ovaries and Fallopian tubes, as compared with those sent to the uterus?

* I have recently had an opportunity of making a cursory examination of the nerves of a virgin uterus, and find they do not differ in size from the nerves of the gravid uterus previously described. Time has not permitted me to carry the examination so far as to determine, whether or not the nerves undergo any increase in length in a first pregnancy.—(June 18, 1846.)

## *On the Nerves of the Uterus.* *By* THOMAS SNOW BECK, *Esq.*

### DESCRIPTION OF THE PLATES*.

Received July 6, 1846.

### PLATE XII.

A. The supra renal capsule.
B. The right kidney.
C. The right ureter.
D. The uterus.
E. The left ovary.
F. The left Fallopian tube.
G. The right ovary.
H. The right Fallopian tube.
I. The bladder.
K. The opening of the divided left ureter.
L. The erectile tissue at the lower and anterior part of the vagina.
M. The vagina.
N. The labia majora.
O. The superior part of the rectum.
P. The anus.
R. The sphincter ani.
S. Some fibres of the levator ani.
T. The clitoris.

*a*. The abdominal aorta.
*b*. The cœliac axis.
*c*. The renal artery.
*d*. The renal vein.
*e*. The left common iliac artery.
*f*. The left external iliac artery.
*g*. The left internal iliac artery.
*h*. The right common iliac artery.
*i*. The inferior mesenteric artery.
*k*. The superior hæmorrhoidal artery.
*l*. A variety of the superior hæmorrhoidal artery which anastomoses with the other trunk of the same artery.
*m*. The point where the two trunks of the superior hæmorrhoidal artery anastomose.
*o*. The divided uterine vessels, arteries and veins.
*p.p.p*. The openings of divided arteries.

1.1.1. The terminal funiculi of the great splanchnic nerve, which are each composed of definite proportions of tubular and gelatinous fibres. The great splanchnic nerve is, essentially, the union of the visceral branches from all the superior intercostal nerves, which are associated with each other in their course to the semilunar ganglion. The branches from the first, second, third, fourth, and fifth intercostal nerves turn down in the so-called trunk of the sympathetic, and after uniting with each other, pass from the trunk of the sympathetic opposite the sixth intercostal nerve, as a large and single branch. The branch from the sixth intercostal nerve turns down in the trunk of the sympathetic, and leaves it opposite the seventh intercostal nerve. The branches from the seventh, eighth, and sometimes ninth inter-

* This description was necessarily delayed until the Plates were lettered.—T. S. B.

costal nerves follow a similar course. Each of these visceral branches of the intercostal nerves becomes associated with a portion of gelatinous fibres, as it passes over the thoracic ganglion of the sympathetic, on a level with the nerve from which the branch arises. Thus the splanchnic nerve, although essentially composed of tubular fibres derived from the cerebro-spinal nerves, has in its formation a portion of gelatinous fibres derived from each of the thoracic ganglia. These two kinds of fibres, *i. e.* the tubular fibres and the gelatinous fibres, although associated together, are yet as distinct as the motor and sensitive tubules of the cerebro-spinal nerves, after they are mixed with each other; for by spreading out the tubules of the splanchnic nerve, each set of fibres can be distinctly and separately traced to its origin and termination. In the course of the gelatinous fibres down the splanchnic nerve, some ganglia are always to be found; sometimes one, when it is comparatively large; sometimes as many as three, when they are small and may be readily overlooked. Were it necessary to offer any confirmation to the fact, that the gelatinous fibres arise in the ganglia of the sympathetic, these ganglia on the splanchnic nerve would offer a ready and convenient demonstration; for by comparing the fibres which enter at their upper part with the fibres which pass out at their lower part, the latter are found considerably to exceed the former. The splanchnic nerve usually exists as a single round cord, having a ganglion at the lower part; but occasionally we find it divided into two very distinct parts, one white and the other gray. In this instance the white part is chiefly composed of the tubular fibres, to which it owes its white aspect, yet it contains a small portion of gelatinous fibres; and in like manner, the gray part, although consisting chiefly of gelatinous fibres, which give it the gray appearance, contains some tubular fibres. Although an interlacing of the tubular and gelatinous fibres occurs in the whole course of the splanchnic nerve, yet it takes place chiefly at the lower part below the ganglion. The apparent object of this interlacing is the due proportioning of the tubular and gelatinous fibres, which is found in the distinct funiculi, in which the splanchnic nerve terminates. The gelatinous fibres of the splanchnic nerve enter the gray portion of the semilunar ganglion; and although I have been unable to trace them further, yet I do not think they end there; for in the sympathetic ganglia, which are so minute as to allow their structure to be examined, without previous dissection of them, the nervous fibres which constitute the nerve upon which the ganglion is formed, all pass beyond the ganglion, after being joined by other gelatinous fibres, which come from the ganglionic corpuscles of which the minute ganglion is composed. The tubular fibres of the splanchnic nerve enter the semilunar ganglion, and after spreading out in its different lobes, pass through it, and are found in the branches of the sympathetic which proceed from the ganglion. Although the semilunar ganglion, from its size and complex formation, is not well adapted for deciding a minute anatomical point, yet I have, by repeated and careful examination, assured myself that the tubular fibres which are found in the sympathetic branches proceeding from it, are all derived

from the cerebro-spinal nerves, through the medium of the splanchnic nerve; and that none of the tubular fibres arise from the semilunar ganglion. Notwithstanding the contrary opinion held by some most distinguished anatomists, viz. that some tubular fibres arise from the sympathetic ganglia, I have, by examination of various parts of the sympathetic, assured myself that the tubular fibres can, in every instance, be traced to the white connecting cord between the spinal and sympathetic nerves; which white connecting cord is a branch from the spinal nerve with which it is connected.

2.2. The lower portions of the lesser splanchnic nerves. They are composed of gelatinous fibres, and tubular fibres, in the same manner as the great splanchnic nerve (1.1). The gelatinous fibres arise from the lower thoracic ganglia of the sympathetic, and passing inwards, are given partly to the renal plexus (9.9), and partly to the superior aortic plexus (21.21, &c.). The tubular fibres are visceral branches of the lower intercostal nerves, and after passing down the trunk of the sympathetic to opposite the nerve next below it, pass inwards, in company with the gelatinous fibres, towards the semilunar ganglion. Some of the tubular fibres turn outwards, and entering the renal plexus (9.9), they are distributed to the kidney; the remainder enter the superior aortic plexus (21.21, &c.), and chiefly pass off at the root of the inferior mesenteric artery (*i*) in the branches of the sympathetic sent from this point.

3.3.3.3.3. The lobes of the right semilunar ganglion, which, in this instance, are much separated from each other. If we restrict the term ganglion to a collection of ganglionic corpuscles, giving rise to the gelatinous fibre, which collection is distinct from another similar collection of corpuscles lying by the side of it, the two being separated by tubular fibres and blood-vessels, then these lobes are an aggregation of distinct ganglia. The tubular fibres of the splanchnic nerves pass through this ganglion and become associated with the gelatinous fibres which arise from it. The association of the gelatinous and tubular fibres constitutes the ordinary branches of the sympathetic.

4. A portion of the left semilunar ganglion.

5.5. Nervous branches which pass between the two semilunar ganglia. I am inclined to think that a crossing of fibres between the two ganglia takes place in this situation.

6.6.6.6.6. Branches of the sympathetic, which pass out with the cœliac axis (*b*), and are distributed to the liver, pancreas, small intestines, stomach, &c. The branches of the sympathetic, of sufficient size to be seen by the unassisted eye, are composed of many gelatinous fibres associated with some tubular fibres; but in the fine divisions of these nerves, small branches are found entirely composed of gelatinous fibres; and if these small branches be further traced, single gelatinous fibres are seen to pass from them, and to enter the cellular coat of small arteries; and finally, to pass along by the side of the artery, taking a slightly winding course round the

vessel, and lying upon the middle coat. The origin and subsequent interlacing of the tubular and gelatinous fibres is better seen in the small branches which pass from the posterior part of the thoracic ganglia. These branches, from their small size, can be examined by the compound microscope without any previous manipulation. At their origin from the ganglion, the tubular and gelatinous fibres are in distinct and separate bundles; and after proceeding a short distance, the fibres composing the two bundles gradually spread out and become interlaced with each other. In some instances, where the small branch bifurcates soon after it leaves the ganglion, the tubular fibres, composing the tubular bundle, divide into two portions, which pass out in the two minute branches in which the small parent branch divides; the gelatinous fibres, composing the gelatinous bundle, likewise divide into two portions, which also pass out in the two minute branches and become mixed with the tubular fibres. The crossing of the gelatinous and tubular fibres in the parent trunk to gain their ultimate destination, is exceedingly well-marked. If the fibres composing these small branches be followed towards the so-called trunk of the sympathetic, we find that the gelatinous fibres take their origin at the corpuscles of the ganglion, and that the tubular fibres are traced through the ganglion to the white cord connecting the spinal and sympathetic nerves. In this case, as in the preceding one, the tubular fibres do not arise from the ganglion, but can be shown to be emanations from the spinal nerves. The origin of these small branches by distinct bundles of gelatinous and tubular fibres, and their subsequent interlacing, bear a striking resemblance to the origin of the spinal nerves, by distinct bundles of motor and sensitive tubules, and their subsequent interlacing.

7.7. Branches of the sympathetic distributed to the supra-renal capsule from the semilunar ganglion.

8. Small ganglion on these nerves.

9.9. The renal plexus surrounding the renal artery.

10. Branches from the renal plexus which accompany a branch of the renal artery to the upper part of the kidney.

11.11.11.11. Ganglia in the course of the renal plexus. Besides these ganglia, there are many much smaller which could not be shown in this drawing, some of which are so minute as to measure but the $\frac{1}{10}$th of an inch in diameter. The gelatinous fibres of the renal plexus are chiefly derived from the semilunar ganglion (3.3.3), though many arise from the ganglia in the plexus (11.11, &c.). The tubular fibres are derived from the lower intercostal nerves through the medium of the lesser splanchnic nerves (2.2).

12.12.12. The ganglia of the abdominal sympathetic of the right side.

13.13. The so-called trunk of the sympathetic of the right side.

14.14.14.14. Branches which pass from the lumbar nerves and trunk of the sympathetic, and enter the superior aortic plexus (21.21.21, &c.).

15.15.15. Three ganglia of the abdominal sympathetic of the left side.

16.16.16. The so-called trunk of the abdominal sympathetic of the left side.

17.17. Two of the so-called gray roots of the sympathetic, which, in consequence of lying loose in the preparation, have been turned in the wrong direction by the artist. These branches, which have often been erroneously considered roots of the sympathetic, pass from the posterior part of the lumbar ganglia, and sending some branches to the vessels in the immediate neighbourhood, join the lumbar nerves. The composition of these branches, as shown by the compound microscope, is analogous to the usual branches of the sympathetic, being formed of many gelatinous fibres, mixed with some tubular fibres. The gelatinous fibres are derived partly from the ganglion from which a branch passes, and partly from the ganglion next above, which latter portion turns down in the cord communicating between the ganglia and then passes out in the branch. The tubular fibres are derived in a similar manner, *i. e.* from the white cord connecting the spinal and sympathetic nerves, which is on the level of the ganglion from which a branch arises, and from the white cord next above. The latter portion turns down in the cord communicating between the ganglia, and passes through the upper part of the ganglion to reach the branch; the former portion leaves the white cord immediately it reaches the ganglion, and turning backwards enters the branch. In this instance, as in the preceding ones, the tubular fibres can be shown to be emanations from the spinal nerves, although they pass through the ganglia in the course of their distribution. The process of tracing these tubular fibres is exceedingly tedious and difficult, yet it can be done; and I have best succeeded by employing the solution of bay salt and arsenite of soda, which, whilst it gives sufficient firmness to the nervous tubules, alters their natural appearance much less than spirit and water. After giving some branches to the lumbar arteries, a portion of the branch joins the lumbar nerves and divides into two parts, one of which passes backwards in the direction of the spinal cord, and the other becomes associated with the gelatinous fibres arising from the ganglion on the sensitive root of the nerve, and passes out in the distributions of the lumbar nerve.

18.18. Two of the white cords connecting the spinal and sympathetic nerves, which have been often erroneously considered roots of the sympathetic. By following the tubular fibres which compose these cords back towards the spinal cord, we find them to be derived from the motor and sensitive roots of the nerve, in apparently equal proportions. And by following the same tubular fibres in the direction of the sympathetic ganglion, we find that on arriving at the ganglion they take several directions. 1. Some turn backwards and enter the gray branch, previously noticed, which passes from the posterior part of the ganglion. 2. Some penetrate the ganglion and enter into the small branches which proceed from the under surface of this body. 3. Some turn down in the so-called trunk of the sympathetic, and supply the tubular fibres found in the different branches which proceed from it; and 4, some, and these by far the most numerous, pass inwards and join the lower part of the superior aortic plexus (21.21, &c.). The last set of tubular fibres becomes apparently attached to

the ganglion on passing over it; but this is caused by gelatinous fibres, which arise from the ganglion, becoming associated with the tubular fibres, and passing with them to the lower part of the superior aortic plexus (21.21, &c.). Sometimes the tubular fibres and gelatinous fibres remain distinct from each other in the whole course of these last-named branches, so that we have a nerve whose upper half is white and whose lower half is gray. These branches, moreover, have a precisely similar constitution to that of the splanchnic nerves in the thorax, and might, not inappropriately, be termed abdominal splanchnic nerves. The two communicating branches between the spinal and sympathetic nerves are differently arranged in the thorax, in the abdomen, and in the pelvis. In the thorax, the branches are of nearly equal size through the whole extent of the cavity. In the abdomen, however, the gray branch is much larger than the analogous branch in the thorax, and gradually increases in size to the lower part of the abdomen. The white branch, on the contrary, is smaller than the corresponding branch in the thorax, and decreases in size towards the pelvis. In the pelvis there is no white branch joining the ganglion; and the branches of the sacral nerves, which might be said to represent it, pass over the sacral ganglia without being at all connected with them, and end in the pelvic plexus. The gray branch, on the contrary, is still further increased in size in the sacral region, and after giving some small branches to the vessels near its course, joins the sacral nerves. The tubular fibres found in this portion of the sympathetic, and in the branches which arise from it, are derived from the tubular fibres of the white branch from the lumbar nerves, which fibres turn down in the trunk of the sympathetic, and becoming associated with the gelatinous fibres form the usual sympathetic branches. This arrangement in the human subject corresponds with that described by Volkman and Bidder in the Frog*.

19.19.19.19. Branches which pass from the abdominal sympathetic to the superior aortic plexus. They are composed of, part tubular, and part gelatinous fibres, in the same manner as the splanchnic nerves in the thorax (1.1. and 2.2). The tubular fibres are derived from the lumbar nerves, and the gelatinous fibres from the abdominal sympathetic ganglia. The purpose of these branches is, moreover, similar to that of the splanchnic, viz. to furnish the tubular fibres which are found in the branches of the sympathetic.

20.20. Small ganglia formed on these branches.

21.21.21.21.21.21.21.21.21.21.21. Ganglia in the superior aortic plexus. The branches proceeding from the semilunar ganglion (3.3.3) to the upper part of this plexus are small, and few in number, and appear merely to preserve the general connexion which exists between different parts of the sympathetic. But the branches at the lower part of the plexus are much larger, and much more numerous, and are formed by the gelatinous fibres which arise from the lumbar ganglia (15.15.15), and from the different ganglia in the plexus (21.21.21, &c.), associated with the tubular fibres,

* Die Selbständigkeit des Sympathischen Nervensystems, tab. 3.

derived from the lumbar nerves, through the medium of, what might be called, the abdominal splanchnic nerves (19.19, &c., and 14.14, &c.). The branches of the lower part of this plexus all converge to one point, viz. the root of the inferior mesenteric artery (*i*), where the association of the gelatinous and tubular fibres occurs, previous to the branches being given off to the large intestines, and to the inferior aortic plexus. From the exact similarity which exists between the semilunar ganglion and the inferior part of the superior aortic plexus, in their formation and constitution, it would not be stretching analogy too far to say, that a second semilunar ganglion exists in this situation, having its elements more apart, and being diffused over a larger space. Indeed these aortic ganglia (21.21, &c.) offer a good demonstration of the formation of the more dense structure of the semilunar ganglion (3.3, &c.). The elements of the semilunar ganglion are, the tubular fibres derived from the intercostal nerves, through the medium of the splanchnic nerves (1.1. and 2.2), and the gelatinous fibres derived from the sympathetic ganglia. And the reason of so large an amount of nervous matter being collected in this situation is, no doubt, from its being a common point whence large and numerous branches pass off to the different viscera. The elements which enter into the formation of the lower part of the superior aortic plexus are exactly similar to those which form the semilunar ganglion; viz. tubular fibres derived from the lumbar nerves through the medium of the abdominal splanchnic nerves (19.19, &c., 14.14, &c.), and gelatinous fibres from the sympathetic ganglia (21.21, &c., 15.15). And the reason why a smaller amount of nervous matter exists here than at the semilunar ganglion is, that the branches which pass from it are much less numerous. However, in estimating the branches which pass from this point, we must take into account not only those which pass out with the inferior mesenteric (*i*) and superior hæmorrhoidal (*k*) arteries, but also the branches which form the inferior aortic plexus (22.22); for this latter plexus is constituted in a manner very different from the superior aortic plexus. Whilst the superior aortic plexus may be said to form a plexus distinct in itself, in which the tubular and gelatinous fibres become associated together to form the branches of the sympathetic, the inferior aortic plexus is merely the continuation downwards of some of these branches, so formed, previous to their dividing to form the two lateral hypogastric plexuses.

22.22.22. Branches of the sympathetic which accompany the inferior mesenteric artery (*i*).

23.23.23. Branches of the sympathetic which accompany the superior hæmorrhoidal artery (*k*, *l*). They proceed from a common point with the preceding branches (22.22), and like them are composed of gelatinous and tubular fibres.

24.24. Branches which accompany the divisions of the superior hæmorrhoidal artery, before the formation of the pelvic plexus (61.61, &c.).

25.25. Branches which pass downwards and join the posterior part of the pelvic plexus (61.61, &c.).

26.26.26.26. The inferior aortic plexus, consisting of branches of the sympathetic

which pass downwards to form the two lateral hypogastric plexuses (29.29). In the middle of this plexus (27) a crossing of the fibres from the opposite sides occurs, by which means the lower part of the plexus, and, as a consequence, the hypogastric plexus of each side, contains nervous fibres derived from both sides of the body; that is to say, if the nervous fibres composing the left hypogastric plexus be followed upwards, a part of them will be traced to the aortic ganglia (21.21, &c.) and lumbar nerves (19.19, &c.) of the left side, and a part to the aortic ganglia (21.21, &c.) and lumbar nerves (14.14, &c.) of the right side.

28. Small ganglion at the termination of the inferior aortic plexus.

29.29. The lateral hypogastric plexus of the left side, which is composed of gelatinous and tubular fibres derived from the lower part of the superior aortic plexus. It supplies the lower half of the uterus, and sends short branches downwards to join the pelvic plexus (61.61, &c.).

30.30.30.30. Fine sympathetic branches which proceed from the inferior aortic plexus and accompany the iliac arteries (*f* and *g*). Three branches chiefly come from the middle of the inferior aortic plexus (27), where the crossing of the fibres from opposite sides occurs.

31. A branch composed of some of the preceding branches (30.30, &c.), which passes in the broad ligament of the uterus to join the lower part of the lateral hypogastric plexus (22.29).

32. A branch composed of some of the preceding branches (30.30, &c.), which also passes down in the broad ligament and joins the nerve (36) distributed to the upper part of the uterus and Fallopian tube. Like the preceding branch (31), it is drawn out of its relative situation by the position of the preparation.

33. Ganglion at the lower part of the lateral hypogastric plexus.

34. The continuation of the lateral hypogastric plexus (29.29) which supplies the lower half of the uterus.

35.35. Nerves distributed to the lower part of the uterus. These nerves are chiefly composed of gelatinous nervous fibres, which fibres form the chief part of the nerves supplied to the uterus. Although some tubular fibres accompany the gelatinous fibres from the hypogastric plexus, and are found in the uterine nerves, yet they are few in number, and appear to be far from forming the essential element of the nerves supplied to this organ. It remains for further inquiry to determine the element of the uterus to which these tubular fibres are distributed, and the function they perform.

36.36.36.36. A branch which proceeds from the lower part of the lateral hypogastric plexus, is joined by a branch (32) from the inferior aortic plexus (26.26), and by two branches (37.37) which come from the nerves between the bladder and uterus. The nerve after the junction of each branch is increased in size, and passes up in the broad ligament to supply the upper part of the uterus (D) and the Fallopian tube (F). It contains both gelatinous and tubular nervous fibres.

38.38.38. A branch of the sympathetic, which proceeds from the inferior aortic plexus (26, &c.), and is distributed to the middle part of the uterus (D), also sending a branch (39) to the ovary (E). The branch passes down by the side of the lateral hypogastric plexus (29.29), but without being at all connected with it in this subject, and then enters the broad ligament passing up to the middle part of the uterus. It is formed of gelatinous and tubular fibres.

39. A branch which is sent to the ovary. Whether any branches come down from the renal plexus, in company with the spermatic artery, or not, I am unable to say, as the preparation was slightly injured in this part; but I am inclined to think that there were branches supplied to the ovary as seen in Plate XV.

40.40.40. Branches which pass from the lower border of the lateral hypogastric plexus to join the pelvic plexus.

41. The lumbo-sacral nerve.

42. Part of the ganglion on the sensitive root of this nerve.

43. The first sacral nerve.

44. The ganglion on this nerve.

45. The second sacral nerve.

46. The ganglion on this nerve.

47. The third sacral nerve.

48. The ganglion on this nerve.

49.49.49. The ends of funiculi which form the fourth sacral nerve.

50.50.50. Portions of the sacral plexus.

51. A branch of the fourth sacral nerve (49.49) distributed to the levator ani (S).

52. Branches from the third sacral nerve (47), which are distributed to the skin of the perinæum and labia majora (N).

53. Some of the same branches, which, being divided, are turned aside.

54.54.54. Branches from the third sacral nerve (47) distributed to the sphincter ani (R) and skin near the anus (P).

55.55. Branches from the fourth sacral nerve (49.49, &c.) distributed to the muscular fibres and skin, at the lower part of the vagina.

56.56. Large branches from the third sacral nerve (47) distributed to the clitoris (T).

57.57. The continuations of the preceding branch (56.56). In consequence of this branch lying across the preparation it became necessary to divide it, and to turn the ends to each side.

58.58. Branches of the sacral plexus which lie loose in the preparation.

59. A branch from the second sacral nerve (45) which joins the pelvic plexus (61.61, &c.).

60.60.60. Branches from the third sacral nerve (47) and from the fourth sacral nerve (49.49), which join the pelvic plexus (61.61, &c.). The branches from the third sacral nerve join the anterior part of the plexus, and are distributed to the bladder and tissues of the anterior part of the vagina, and those from the fourth sacral nerve

join the posterior part of the plexus, and are distributed to the erectile tissue and tissues at the posterior part of the vagina, and also to the rectum.

61.61.61.61.61.61. Small ganglia in the pelvic plexus. They are situated at the point of union between the branches from the lateral hypogastric plexus (29.29) and the branches from the sacral nerves (60.60.60). The lower part of the lateral hypogastric plexus forms a continuous plexus with the pelvic plexus, and the only circumstance which marks the division between the two is the existence of these ganglia, and the junction of the branches from the sacral nerves. It appears, however, from the marked difference which exists in their constitution, necessary to make this distinction between the two plexuses. For whilst the lateral hypogastric plexus (29.29) is a continuation of branches from the lower part of the superior aortic plexus (21.21, &c.), the pelvic plexus (61.61, &c.) is a point where a new arrangement of the gelatinous and tubular nervous fibres occurs, previous to the branches being distributed to the bladder, vagina and rectum. In this respect the pelvic plexus is analogous to the semilunar ganglion (3.3, &c.) and to the lower part of the superior aortic plexus (21.21, &c.), from which it differs in the amount of nerves sent from it, and the large amount of tubular fibres which these nerves contain. This analogy is further shown by the branches from the sacral nerves (60.60, &c.) being analogous to the abdominal splanchnic nerves (19.19 and 14.14), and to the thoracic splanchnic nerves (1.1. and 2.2), as being the medium by which the tubular fibres are distributed to the viscera. The sacral branches however differ from the splanchnic nerves in not being associated with any gelatinous fibres. These branches pass over the sacral ganglia of the sympathetic without having any connexion with them. In the branches of the sympathetic proceeding from the semilunar ganglion (6.6, &c.) and from the superior aortic plexus (22.22, &c.), and distributed to the abdominal viscera, the amount of the gelatinous fibre greatly exceeds that of the tubular; but in the branches from the pelvic plexus (61.61, &c.), distributed to the bladder and vagina, the amount of tubular fibre exceeds that of the gelatinous; and in the branches sent to the lower part of the rectum (68.68), the amount of tubular and gelatinous fibre is nearly equal. The gelatinous fibres found in the pelvic plexus are derived from the lateral hypogastric plexus, augmented by the gelatinous fibres which arise from the small ganglia (61.61, &c.). The tubular fibres are derived from the sacral nerves, though some tubular fibres from the lumbar nerves enter the plexus with the gelatinous fibres from the lateral hypogastric plexus.

62.62.62.62.62. Small secondary ganglia which are formed in the pelvic plexus, and which assume various and curious forms.

63.63.63.63. Branches from the anterior part of the pelvic plexus, which are distributed to the bladder. They contain a large amount of tubular nervous fibre.

64. Small ganglion on the vesical nerves.

65.65. Branches which come from the fourth sacral nerve, and joining with branches from the lower part of the pelvic plexus, are distributed to the erectile tissue (L).

These sacral branches pass down by the side of the pelvic plexus, but have no communication with it.

66.66.66.66. Small ganglia formed on the nerves before they enter the erectile tissue.

67.67.67.67. Branches which enter the erectile tissue at the lower and anterior part of the vagina (L).

68.68.68. Branches which come from the posterior part of the pelvic plexus (61.61, &c.), and are distributed to the rectum.

## PLATE XII.*.

A finished engraving of the preceding plate.

## PLATE XIII.

Showing the posterior part of the pelvic plexus, which lies upon the rectum. This portion of the plexus is concealed in Plate XII. by the sacral nerves (47 and 49.49), and by the branches of the sacral nerves (59 and 60.60) lying upon it.

A. The upper part of the rectum.

B. The sphincter ani.

C. Fibres of the levator ani.

D. The two trunks of the superior hæmorrhoidal artery, as in Plate XII. The artery divides into branches which are distributed over the surface of the rectum.

1. A portion of the fourth sacral nerve.

2. A branch from the fourth sacral nerve sent to the levator ani.

3.3. Branches from the fourth sacral nerve which enter the posterior part of the pelvic plexus.

4. Three branches which come from the lower part of the superior aortic plexus (21.21, Plate XII.), and accompanying the superior hæmorrhoidal artery, end in the posterior part of the pelvic plexus.

5.5. Branches from the superior aortic plexus, which accompany the superior divisions of the hæmorrhoidal artery, before the formation of the pelvic plexus.

6.6.6. The divided ends of branches which enter into the formation of the pelvic plexus.

7. A branch distributed to the vaginal erectile tissue.

8. Small ganglion on this branch.

9.9.9.9. Branches from the pelvic plexus, which are distributed to the rectum. These branches follow the course of the arteries for some way, but leave the vessels in their ultimate distribution to the gut. They consist of gelatinous and tubular fibres in nearly equal proportions.

10.10. Small ganglia on the hæmorrhoidal nerves.

## PLATE XIV.

A. The gravid uterus at the ninth month of pregnancy.

B. The right Fallopian tube turned aside.

C. The right ovary.

D. The right kidney, which has been cut at the lower part to show the structures beneath.

E. The right ureter.

F. The bladder.

G. The vagina.

H. The rectum.

I. A portion of the abdominal aorta.

K.K. Two renal arteries.

L. The renal vein.

M. The spermatic artery.

N. The continuation of the spermatic artery to the fundus of the uterus.

O. The division of the spermatic artery which supplies the ovary.

P. The superior hæmorrhoidal artery covered with fat. The nerves which accompany it are not dissected.

R.R. Portions of the superficial layer of muscular fibres which have been dissected out by Dr. Robert Lee, and described by him as nervous ganglia and plexuses. These muscular fibres adhere to the under surface of the peritoneum.

S. The edge of the divided peritoneum.

1.1. Portions of the semilunar ganglion.

2.2. Portions of the renal plexus.

3. The aortic plexuses.

4. The lateral hypogastric plexus.

5. A continuation from the lateral hypogastric plexus, which is distributed to the superior part of the bladder.

6.6.6.6. Continuations from the lateral hypogastric plexus distributed to the lower half of the uterus.

7. A nervous branch which supplies the middle part of the uterus. It is inclosed in a tendinous canal, which extends to the bifurcation of the branch, and is apparently analogous to the branch described at 38. Plate XII. The difference which may at first appear to exist between these two branches passes away, when we remember that the lateral hypogastric and inferior aortic plexuses are both emanations from the superior aortic plexus.

8. A portion of the preceding branch, which has been divided in the dissection. It lies loose in the preparation.

9. A branch which accompanies the spermatic artery, but which was divided in removing the preparation from the body.

10. Part of the sacral plexus.

11. Branches which enter the middle and anterior part of the pelvic plexus.

12. A branch which joins the posterior part of the pelvic plexus.

13. A branch from the sacral nerves distributed to the muscular tissues at the lower part of the vagina.

14. Branches which come down with the superior hæmorrhoidal artery and join the posterior part of the pelvic plexus.

15.15.15.15.15. Small ganglia in the pelvic plexus. Several smaller ganglia also exist in this plexus.

16.16.16. Nerves distributed to the bladder from the pelvic plexus.

17.17.17. Branches sent to the erectile tissue at the lower and anterior part of the vagina.

18.18.18. Branches from the posterior part of the pelvic plexus distributed to the rectum.

The formation and composition of the nerves in this dissection are, in all cases, exactly similar to that described in the previous dissection, Plate XII. Some of the branches, however, have been broken in consequence of my beginning the dissection with opinions very different from what I afterwards found to be correct.

## PLATE XV.

Showing the distribution of the Nerve to the ovary and fundus of the uterus on the opposite surface of the same gravid uterus figured in Plate XIV.

A. The fundus of the gravid uterus at the ninth month of pregnancy.

B. The left Fallopian tube.

C. The fimbriated extremity of the Fallopian tube.

D. The left ovary.

E. A large vein.

F. The left spermatic artery.

G. The continuation of the spermatic artery to the fundus of the uterus.

H. The branch of the artery distributed to the ovary.

I.I. Another portion of the superficial layer of muscular fibres, which has been described as nervous ganglia and plexuses by Dr. Robert Lee.

K. The edge of the divided peritoneum.

1. A branch of nerve which proceeds from the renal plexus in company with the spermatic artery, and is distributed to the ovary and fundus uteri.

2. A fusiform enlargement formed on the nerve previous to its distribution.

3.3. Branches sent to the ovary.

4. A branch sent to the fundus of the uterus.